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MADISON BENTLEY, UNIVERSITY OF ILLINOIS

HOWARD C. WARREN, PRINCETON UNIVERSITY (*Review*)
RAYMOND DODGE, YALE UNIVERSITY (*Monographs*)
S. W. FERNBERGER, UNIVERSITY OF PENNSYLVANIA (*Bulletin*)
WALTER S. HUNTER, CLARK UNIVERSITY (*Index*)
HERBERT S. LANGFELD, PRINCETON UNIVERSITY, Business Editor

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THE TWO-STORY DUPLICATE MAZE

TRACING THE STYLUS MAZE WITH A MAXIMUM OF INDIRECT VISUAL GUIDANCE

BY WALTER MILES

Psychological Laboratories, Stanford University

VISION, THE COACH OF ACTION

In the realm of motor performance most individuals need not be told to 'look before they leap,' for action is inhibited unless accompanied or preceded by visual cues. Vision is the critical supervising partner in nearly all perceptual situations. The late Professor Sanford characterized it, in his well-remembered lecture on 'The function of the several senses in the mental life' (1), as the general business sense of the mental world. Not only can it serve us directly or indirectly in place of other sensory modalities, but the fact is we are rather uncomfortable unless it is so serving. If conditions do not permit us to see the object of our attention, nevertheless the task is better performed (Johnson, 2) if we have our eyes open and are receiving light.

It is vision that really persuades the performing individual that his movement is of the right kind and appropriately accurate. The young debater learning to gesture or the careful adult in his first practice of the hand signals for motor driving is apt to look at his hand. He is not quite ready fully to trust the kinæsthetic impressions; or, perhaps better, he is so in the habit of constantly supplementing these by sight that in this case he acts oddly.

Vision provides us a comprehensive, ultra-rapid, spatial survey of our surroundings and of our own location. It is somewhat as though one could make use of a score of right hands to touch with all at once. But these hands would not cover every direction and there would be untouched gaps between them. The eye 'touches' all that is

compassed by its wide angle and a minimum of spatial patchwork is required of the observer. Thus seeing is localizing and there is no other experience of position that is quite the equal of that which is got in terms of simultaneous, synergistic configuration that we call the visual field. The clearness and precision of visual data constitute a unique feature of the optical sense. "Beside it the other senses are dull and groping" (1). Blindness is really a worse handicap than was once thought, for evidence accumulates (Koch and Ufkess, 3) that vision provides a factor in the comprehension of spatial relations that cannot be replaced by anything else, no matter what the training.

No delays in action are occasioned by vision; it does not get in the way or occupy valuable space; it does all its coaching unobtrusively and from the side. It is quite a different thing from having another hand to use on the object, or having another witness present in the room. Without the slightest conflict, the same object can serve simultaneously as stimulus to vision while it is worked with by the hand or other muscle group. Woodworth (4), in considering the sensory basis for the control of movement, admits that any sense may conceivably serve for controlling the extent of movement. He states that vision and hearing are the most accurate "largely because the breadth of their sensational fields makes possible a *superposition* of two sensations and so a more exact comparison than can be made where memory has to be relied on for one sensation. We can see the target *while* we hit at it; we can hear the pitch *while* we attempt to strike it. But we cannot *feel* a certain length or direction of movement while we attempt to reproduce it" (Woodworth, 17, p. 72). Reaching for an object that we can see is not unlike sliding the finger tip (with eyes closed) along the surface of the body toward a well-known point; *e.g.*, along the forearm and wrist to the base of the thumb nail. Such a movement is in contrast to our efforts at finding the light in a wholly dark and unfamiliar room, or in finding the exit of a stylus maze that is completely strange and hidden from view.

Forms and Degrees of Visual Guidance

When stating the conditions of an experiment that demands motor performance, it is, as a rule, described as 'with vision' or 'without vision.' But the range of conditions is really quite wide from working in a strange room in absolute darkness to having the task 'at the most comfortable reading distance,' unobscured, and illuminated with the best north day light. Both extremes of this scale are seldom realized in practice and probably need not be. There are several intermediate stages which are sometimes provided as conditions to be purposely experimented with for discovering the rôle of vision in various psychological settings.

The forms and degrees of visual guidance available as experimental conditions may be roughly outlined as follows.

1. *Full Normal Visual Control.*—This means adequate illumination, comfortable position not involving special muscular strain, and full opportunity for seeing as the subject may wish. It is assumed to be, unless otherwise specified, the condition

present in experiments on motor performance. It would apply to tests of writing, accurate tracing, paper and pencil maze, aiming, pursuit movements, serial reaction to visual stimuli, and the like.

2. *Interrupted or Fragmentary Visual Control.*—A brief view as by a lighted match or a flashlight gives, as compared with darkness, much guidance for action. The old experiment of Bowditch and Southard (5) is pertinent here. Their subjects got the location of a point by looking at it and then with eyes closed tried to pierce the point with a stylus. A simple arm-movement was all that was necessary; but they usually missed the point by 12 mm. If the point was first located by touching it with the other hand, the average error was about 20 mm. When there are many features to be watched and they can not all be well taken in at once, as in auto driving or in working on Dunlap's "LVN apparatus" (6), fragmentary visual guidance may be said to exist. Handwriting, walking, and many motor habits are to be grouped here.

3. *Partial Visual Control.*—Representative conditions that would operate to make the visual control poorer but not different in kind include dimness or glare of light, blurred visions from pressure on the cornea or the action of such substances as atropin or cocaine, disturbances of central vision by scotoma and other pathological factors. Probably this sort of visual guidance is seldom worked with as an arranged condition, but as a rule applies rather to "off" days and periods, and to aged and pathological groups.

4. *Remote Visual Control.*—A task may be difficult because the work is too far from the eyes or the details so small as to need magnification. A jeweler's motor coördinations would be clumsy if he had to work without his loupe. When one looks through an ordinary binocular or opera glass in the reverse of the usual direction, the field of view appears small and distant. If one will watch in this manner his hand on the sheet of paper while writing, one will gain a vivid experience of remote visual guidance. By using a glass with a magnification of 6 or 8 diameters one will probably find that the specimen of handwriting will stand about midway between his normal and a sample that he makes when writing rapidly with closed eyes. Experiments with uniform tempo but with magnification or reduction as compared to seeing normal size might profitably be made.

5. *Distorted Visual Control.*—By the use of mirrors, prisms, or other refracting agencies, it is easily possible to modify the visual appearance so that there will be considerable disagreement between it and tactual-kinæsthetic impressions. Researches using such modified visual guidance are represented by Starch (7) with mirror drawing, Stratton (8) in his eight-day experiment in wearing prismatic glasses which inverted the visual field, and more recently by Wooster (9) who had her subjects wear for short periods prismatic glasses that produced an angular deviation of about 21 degrees. A very common experience that often involves this factor is reaching for objects that are under water. The field is an attractive one for experimentation as it brings into relief the rôle of vision.

6. *Indirect Visual Control.*—One form of indirect visual guidance is the fixation of a point considerably removed from that to which attention and manual manipulation are applied. Motor control under this condition has been examined, as for example in the work of Bowditch and Southard (5) mentioned above. The average error made when attempting to place the stylus on the point which had been thus seen in indirect vision was 15 mm, which is 3 mm greater than the error made when central vision was used. The difficulties of controlling fixation are too obvious to need mention

here. Indirect visual guidance is more strictly a condition such that the subject *cannot see* the object on which he is to work out his motor coördination, but provision is made for him to look directly at another duplicate object or diagram from which he can gain information that will assist him in his movements. Professor Carr's exceedingly clever stylus maze with the hidden stops in the lower wide slot (10) enabled him to study the influence of both direct and indirect visual guidance on maze learning. To the left of the covered maze, which the subject was to work on with his arm through the screen, a diagram of the maze was placed in the same relative position. The subject could study this diagram as he made his way through the maze that was screened from view. Diagrams of various degrees of completeness were used. Although indirect was not as good as direct visual guidance, still the subject learned much more rapidly when a diagram was visible. With the complete diagram the saving in number of trials amounted to 62 per cent. and the saving in errors to 93 per cent. (Table VI, p. 411).

7. *Non-visual Control.*—Objective conditions that properly qualify here are complete darkness, complete blindness, and perfect blindfolding. In work with blind subjects the application of the blindfold probably should not be omitted for reasons of maintaining standardized conditions. Ordinarily the blindfold is somewhat uncomfortable and distracting; if it is therefore applied to one group it should be to all. In maze experiments, as an example of motor tasks performed without vision, it is common to screen the maze (*a*) by a canopy covering the top of the table on which the maze rests (Warden, 12) or (*b*) by a screening booth ("hooded maze") open at the back (Carr, 10; Koch, 13; Ludgate, 14) so that the experimenter can observe. In front the cloth hangs loosely to form ample cover for the arm that is thrust through. When the maze is screened the subjects work in a well-lighted room, have their eyes open, and look at what they will. To speak strictly, this is not a non-visual condition. To be sure the subject is not provided with a map or diagram to start with; through his tactual experiences coupled with perception of hand and arm positions he has, however, the opportunity of building up quite an elaborate visual scheme from the objective surfaces before him. The condition surely offers advantages over working in complete darkness or wearing a thoroughly comfortable but complete blindfold. It is beyond the province of this paper to consider visual imagery, and we shall turn now to a description of the two-story maze for working with indirect visual guidance under various conditions of maze rotation.

The Two-Story Maze Apparatus

The two-story maze to be described in this article was first arranged in 1917 (Miles, 11), not so much as a learning experiment as for a short motor test "in which the subject, while looking at something which corresponded to a map, would be required to carry out a series of movements in accordance with definite rules." Its object was to provide a fairly standard motor task where the conditions for indirect visual guidance were made as complete and satisfactory as possible. Such arrangements, which divorce vision and

tactual-kinæsthesis from their intimately coöperative behavior, may serve to differentiate subjects and possibly provide a diagnostic test for selecting individuals for certain kinds of task.

The nature of our equipment can be easily understood from Fig. 1. The essential feature consists in duplicate stylus mazes, one of which is placed directly above the other.

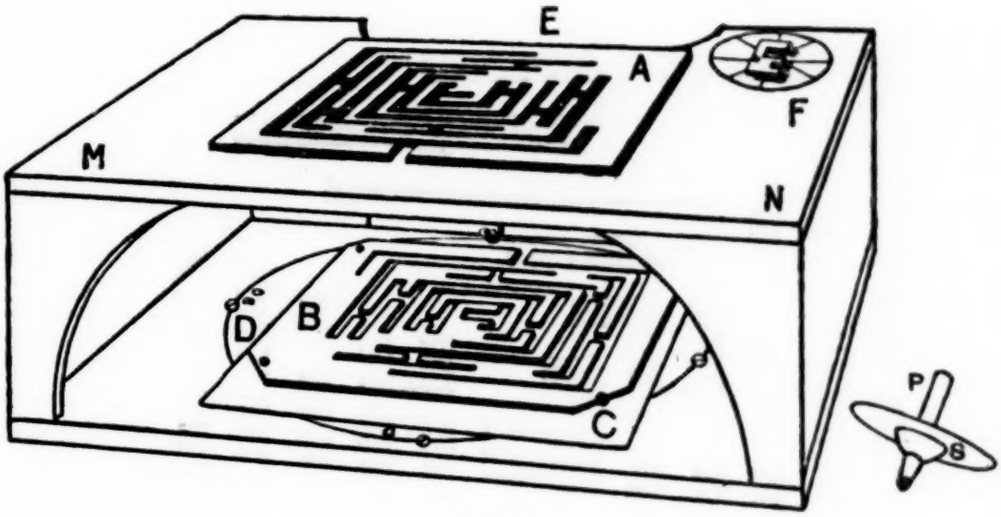

FIG. 1.

The mounting of the mazes is provided for by a wooden box with two of its sides cut away. The front, as shown, is so opened that the subject can place his hand and arm inside for tracing maze *B* with a stylus or pencil. (So far we have the typical screened maze situation.) The top of the box which hides maze *B* carries maze *A* n full view. A strip of cardboard is fastened to the box edge *MN* and, projecting toward the subject, completes the screening of his arm and hand. The back side also is cut away, so that the experimenter may watch the movements of the pencil in tracing maze *B*. Observation is facilitated by removing a portion of the top of the box as shown at *E*. The box is hinged on the back side so that if desirable the top and sides in one section may be turned up, thus uncovering maze *B*. Maze *A* is fastened permanently by four small screws. The lower maze has a small hole at each corner and fits on four studs that are secured to the metal disc *D*, which disc can be revolved. In the figure it is shown revolved 180 degrees from the normal position. A sheet of paper, *C*, placed on the studs before the maze is put on provides for a record of the tracing movements. The pencil *P* carries a wide rubber guard *S* so that the fingers will not touch the maze. The inside clearance between maze *B* and the top is 11 cm, a distance that is ample for the hand in manipulating the short pencil. It is desirable to have the box shallow to obviate a great discrepancy between the convergence of vision and the perceived distance of the hand.

On the top and in full view is an index *F* for assisting the subject in comprehending the exact position (degree of rotation) of the lower maze. This index consists of a small metal disc on which is mounted a duplicate of the very central bit or starting

section of the maze. This index is rotated to correspond with the amount of rotation of maze *B* and thus gives the subject a direct hint of the first move to be made. Maze *B* may not only be revolved, by turning the disc which carries it, but may be inverted as well. In every position it is practically immovable when in use.

The two mazes are of a pattern that employs only right angles. They were cut from aluminum sheets 3 mm thick. The paths are 6 mm wide and 6 mm apart. The sides of the paths present very few cues by reason of roughness. The exact form of the maze pattern is shown in Fig. 2. The longest straight paths are 16 cm each.

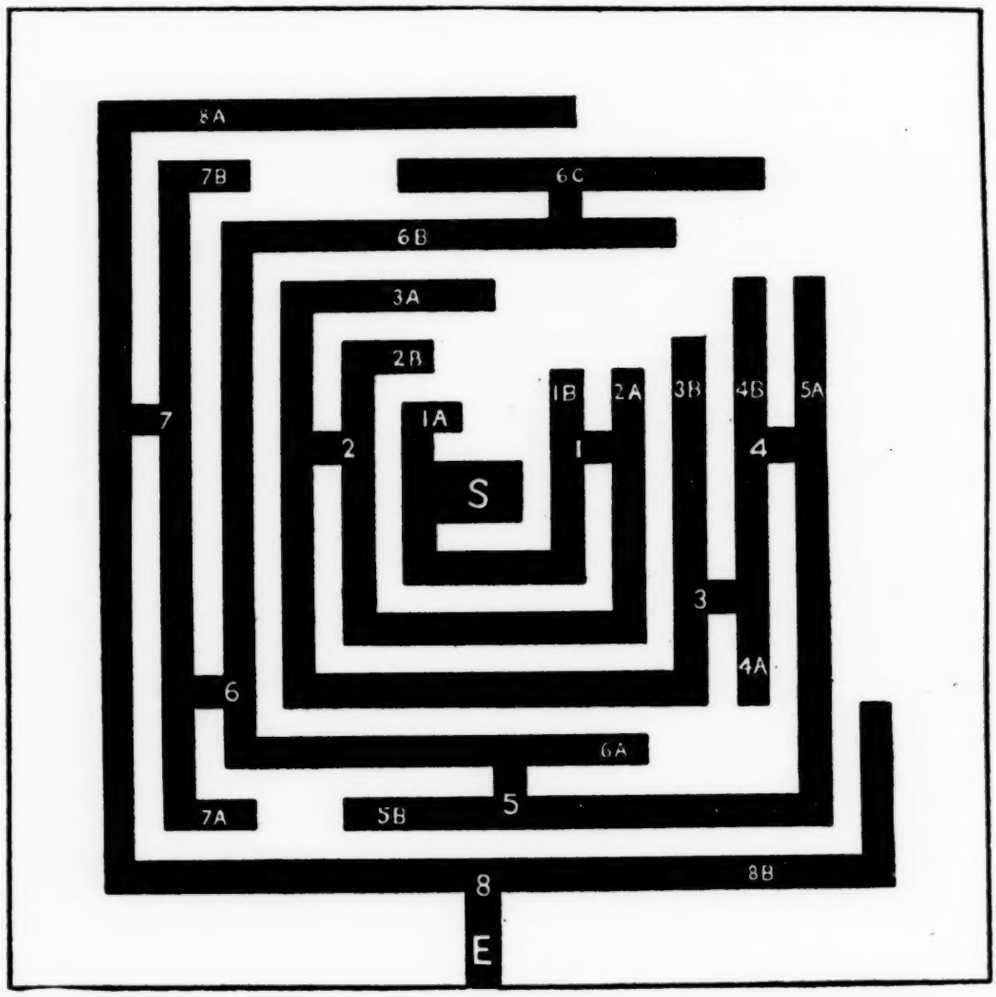

FIG. 2.

The pattern selected is one that had been used by Boring (15). It has the advantage of being square, built on the concentric principle with progress to be made from the inside out (with one conspicuous exception), and all the sections which divide the paths have fairly secure natural anchorage. The culs-de-sac are not as variable nor as devious as is the case in some maze patterns. Visually the true path may be carefully gone over in about 5 seconds. It is nearly one meter in length, requires the finding and passing of 8 openings, and involves a total of 25 turns. It will be noted from the diagram that the true path does not enter the upper two-fifths of the figure. No claim is made that this pattern is the best possible for the two-story maze. With

some subjects a more difficult pattern would be serviceable; but most persons find this simple diagram sufficiently hard for those maze problems which are to be solved at one sitting and there are some presumably normal adults for whom this pattern is too difficult.

Representative Results for the Two-Story Maze

In starting work with a subject on this maze it is first explained to him that the two parts, *A* and *B*, are just alike, and *B* is lifted up and superimposed on *A* to demonstrate this fact. The paper to receive the tracing is put in place and *B* returned to its proper position. It is then explained that the subject is to trace the true or shortest path through the top maze with a wooden stylus in his left hand at the same time that he uses the pencil in tracing the hidden maze below. He is instructed to make each hand proceed at the same rate and execute a given part of the pattern at the same time. He is cautioned against getting lost in *B*. If at any time he is lost, he should ask to have his hand returned to the center for a fresh start. Time is taken from the first beginning until he makes a successful exit. The reason for the guard on the pencil is explained and the index (*F*) is shown and its significance pointed out. When *B* is rotated from the normal position, special care is taken with the aid of the index to explain the degree of rotation.

Simply to trace through maze *A*, in full view, with a small wooden stylus held in the right hand requires an average of about 18 seconds. This means, of course, that the subject sees nothing of the pattern until his preferred hand holding the stylus has been placed in the starting position ready for him to begin actually tracing the moment he opens his eyes. It is a very simple task and no normal adult who has vision sufficient for reading could fail in it. Some quick adults perform it in 10 seconds and do not make the slightest entrance into a blind. Others take as long as 40 seconds and may get the stylus well into or even to the far end of one or more blinds. The hand that is doing the tracing necessarily hides a part of the pattern. The exit is next to the subject and may be covered by his wrist. The hindrance to vision probably accounts for many of the errors and slow times.

This kind of maze-tracing is somewhat like the paper-and-pencil maze experiments (Porteus, 16); but comparable data for a paper maze and a stylus maze of the same pattern and size are not at hand. The slot maze provides mechanical guidance and therefore should be considerably easier when in view than the printed pattern where nothing but visual guidance can serve at any moment.

When both mazes, *A* and *B*, are in the same relative position, which may be defined as 0° rotation of *B*, and are traced simultaneously with *A* in full view and *B* completely hidden, the average time required for *B* is about 90 seconds. This is five times as long as for the simple tracing of *A* in full view.

Fairly often in this maze problem subjects require a new start. They frequently enter blinds and a few subjects get almost hopelessly confused; but I do not know of a single university student actually failing in the problem, although some have required 5 to 7 minutes for its completion. As put to the subject, the problem is of course absolutely different from working a maze under blindfold conditions. But it is not impossible that some subjects, although they move the wooden stylus as they should in *A*, largely neglect making use of indirect vision and proceed in a random manner. With most subjects it is definitely a process of checking and identifying visible items (corners and distances) with items felt with the pencil. Where the subject lacks assurance he may retrace a distance or run into a short blind near by to verify the position, much as one might do with a map and a guide book in a foreign city. He gets into difficulty by pressing his pencil against the wrong side of the path, the capricious element in all stylus maze work, and thus fails to find an opening at a place "where it should be." If he slips through an opening without realizing it, thinking that he has changed to the other side of the path, a long delay is likely to result. Opening 5 (see Fig. 2) naturally gave the most frequent difficulty.

The task in general may be described as involving indirect visual guidance at its best, or maximal, efficiency. The two conditions which make possible this maximal use are: (1) having the visible pattern an exact duplicate of the maze that is being traced rather than a paper diagram, and (2) having it located in reference to the hidden or screened maze so that the positions of the head and eyes are as little different as possible from what they would be if the subject could look right through the screen at his hand with the pencil. Under such conditions the task is really quite simple and can be very rapidly learned. Occasionally subjects complete the first

trial in from 25 to 30 seconds, and Dr. Cox[1] had one in a group of about 70 who made a score of 18 seconds. As a rule the second trial at this problem requires only about half as long as the first.

A more difficult problem, but not really so difficult as it may seem to the person without experience, is to have maze *B* rotated 180 degrees so that the relative positions of *A* and *B* are as shown in Fig. 1. This is not like mirror-drawing: it is less difficult, even though the tracing hand is entirely out of view. The mirror reverses movements that are on the axis perpendicular to itself but not those that are parallel to its surface. It is this mixture that makes the first attempt at mirror-tracing difficult, particularly when the line to be traced is diagonal to the plane of the mirror. In the present maze problem there is a complete reversal as compared with the pattern on *A*. The hands must be made to move in opposite directions from start to finish. The solution of this problem is probably hindered somewhat by mental set. Subjects expect it to be difficult. More factors have to be constantly watched; and it is found to require three or four times as long to complete the first trial as to do the 0° problem. The subject must be cautioned to keep the rule of reversal constantly in mind. Failures will occur on this problem. In a group of 67 college men 2 failed entirely after trying 18 and 33 minutes. Another group of 26 college men gave one failure.

This fellow, who was expecting to be a physical director, tried for 30 minutes with many fresh starts the first time. Two weeks later he succeeded in getting through with a score of 432 seconds, but on eight later trials, separated from each other usually by a fortnight, he did not succeed once (11, Table 187). Dr. Cox found failures somewhat more frequent. As a rule she terminated trials at the end of 15 minutes.

The group of 65 men who solved the 180-degree problem gave a range of 58 to 1,150 sec with an average of 264 sec. The average for the second trial, which immediately followed the first, was 119 sec.[2] The group of 25 men shows a mean for their first trial of 290 and a range from 106 to 976.

[1] Dr. Catharine M. Cox used the two-story maze in an investigation at Stanford University on the relation of intelligence to the solution of maze problems. It is expected that her material will soon be published.

[2] The correlation of these two consecutive trials for the 65 men is + 0.50 by rank order.

Some comparative individual results for problems involving various positions of the lower maze, the upper maze remaining fixed, are given in Table I. For the six subjects, three men and three women, for whom data happen to be at hand, it is obvious that the 0° position gives the lowest and the most uniform results. A position of 90-degree counterclockwise rotation seems much harder than that of 180°. All six subjects give this result, and there is also some indication that it is harder than a similar amount of rotation (270) in the opposite direction. A 45-degree rotation is apparently the easiest thing, with the exception of 0°; and for four of the subjects it is definitely easier than the 180-degree position. The problems that involved rotation and inversion are not, as a rule, as difficult as the quarter-turn or 90° positions.

Table I

Comparative results on problems with the two-story maze

(Time values in sec)

	Amount of Rotation of Maze *B* with Reference to *A*						
Subjects	0°	180°	90° [1]	270° [2]	45° [3]	180° [4]	270° [4]
E. D. S. ♂	35	80	389	319	79	252	527
C. P. F. ♂	24	124	171	240	67	74	220
R. H. T. ♂	57	191	329	107	94	149	63
M. C. B. ♀	51	212	489	287	170	94	178
A. W. ♀	39	72	585	480	85		
W. B. ♀	32	246	305	882	105		

[1] Turned 90° counterclock. [2] Turned 90° clockwise. [3] Turned 45° counterclock. [4] Rotated the given amount and then inverted so that the part that had been near 180 on the disc was now near zero.

General Discussion and Summary

Tracing through a stylus maze in full view is a perfectly natural sort of situation and can in no way be made easier except through prior instruction or direct motor learning. In fact it is so simple as almost to come under Woodworth's classification of 'action that is too easy' (17). Vision supplies the spatial pattern and the localization, and the hand has but to follow along and is even aided, under our conditions, by the mechanical guidance of the slots. This coöperation between eye and hand is immediate both temporally and in

space. Vision 'goes' so rapidly that it can explore a wide area and still keep the hand continuously moving. Nothing is more representative of human behavior than this facile coöperation. At the other extreme, we have real 'maze' work; that is to say, tracing without any visual assistance. It is described as "trial and error" and involves extended trials; for there is no straight-forward, errorless kind of procedure that may be followed. The hand and arm have, under these conditions, no capability that is adequate. It is 'trial' also in the sense of being more or less annoying as a task. Maze performance in a noteworthy way displays emotional factors. Between the simple task with vision and the blind task there would seem to be an opportunity for arranging problems of intermediate difficulty. These may be used as objects of study in themselves or for purposes of correlation. Such problems have the advantage of being relatively new and untried for most individuals. They give a favorable opportunity for observing behavior. The problems proposed are not maze tasks as usually understood. They more closely resemble such things as the Porteus tests of intelligence (16). Perrin (18), Koch (13), and others have used rotation of the maze but chiefly as a distraction and to examine the retention and adaptation of learned material.

A very frequent comment by the subjects was that the distances on the hidden maze seemed much greater than those that were presented to view. Subjects complained that the pencil was 'a poor thing to feel with.' The tip of the finger would doubtless be much more efficient and would add a factor of spatial-area to the experience of contact when no movement was being executed. The appraisal of the difficulty of the various problems differed somewhat from subject to subject; but in general agrees with the time-scores. A 45-degree turn can be subjectively adjusted-for as a mere inaccuracy of placement; but a turn of 90 degrees cannot be so dealt with. The rule of action is here more complex than that which applies to the 180-degree position; and subjects more often disregard visual cues, trusting to get through somehow.

1. Various degrees of visual assistance may be used when tracing or manipulating objects with the hands and where accurate coördination is required.

2. Maze-tracing as ordinarily done behind a screen is not a truly non-visual performance.

3. An arrangement of two duplicate mazes is described which permits of having one maze in full view and the other directly below it and screened from view. The lower maze is to be traced while the upper maze is followed visually, a condition of maximal indirect guidance from vision.

4. A comparison is made of results for various two-story maze problems. A rotation of 180° in the lower maze requires about three times as long to trace as the register position, 0°. A quarter-turn (or 90-degree rotation) is much more difficult than 180 degrees, while the 45-degree turn is much easier.

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INSIGHT IN THE WHITE RAT

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Until very recently all questions concerning the intelligence or learning of animals were answered in terms of certain theories which have gained wide assent among psychologists and have been applied almost universally in educational circles. In fact it is not too much to say that the majority of workers in the field of animal behavior have been dominated by the fundamental assumptions expressed or implied in the theories of Thorndike and Watson.[1] Both of these theorists tell us, in effect, that animal, and by implication, human learning is nothing more than a concatenation of reflexes knit together in serial order. While Thorndike assumes that pleasure and pain may affect learning and Watson gives some place to recency, certain it is that neither these psychologists nor those who follow them provide in any way for learning in accordance with the sensible demands of a situation. So plausible did the chance theories of learning seem until the advent of Köhler's work with the apes, except for an occasional observation here and there pointing in another direction but hardly inspiring us with a new point of view, that it now appears as if most of the experiments performed on animals in this country were designed chiefly to supplement and strengthen what seemed to be valid and fruitful assumptions.

Even when experimenters have attempted to decide strictly the limits of ability of members of a species or of one species as compared with another, the general assumptions underlying the experiments and the setting of the experiments themselves have nevertheless favored answers conforming to the trial and error, chance and conditioned-

[1] Although Thorndike and Watson differ on fundamental points which lead ultimately to quite different positions, the theories of both are based upon an atomistic view of behavior. The differences between the two will be mentioned where they are important for our discussion. Thorndike's views will be found in his *Animal intelligence*, 1911, and Watson's in *Behavior, an introduction to comparative psychology*, 1914.

reflex theories. Much of the experimental work having been done upon the white rat in the maze, a large literature exists showing how this animal works blindly and mechanically at its problems, needing only time, kinæsthesis, and chance to stamp out the wrong responses and to stamp in the correct.

It would probably be admitted by everyone that it is bad logic to generalize from some cases to all cases and from one set of conditions to all conditions, yet this is precisely what has happened in the field of learning. Conclusions based upon chance conditions have been transferred to learning under all conditions. It is the contention of this paper that if an animal is subjected to an experiment in which only chance results can appear, the results may be made to fit into trial and error and frequency-recency hypotheses, although even under these conditions it is often hard to explain the actual behavior of the animal and the expected does not occur; conversely, if the experiment is designed to test the insight or intelligence of the animal, little as it may possess, the results will reveal something besides blind, stupid effort directed with no sense of ends or goals as determining factors in the resulting behavior. In other words, if we present the animal with a situation which it can survey more or less (and this requires thought and labor, for we cannot tell in advance to what aspect of a total situation an animal will respond), we venture to predict that its learning will not be amenable to the simple theories based on the concept of the reflex as the unit out of which all complex behavior is built.[2] If the animal responds to the total situation (structure-function) then the theories which imply that its behavior is directed toward, and conditioned by, 'elements' in the field must be more critically examined. If the part activities or single reflexes are explicable only in the light of the end to be reached, the total act, or the sense which the situation has for the animal, then part does not join to part activity because of chance contiguity and we can no longer regard instinct and habit as merely chain reflexes knit serially together. Finally,

[2] It now appears as if the concept of the reflex will not explain even the most elementary facts of physiology, to say nothing of the facts it has been applied to in psychology. Yet psychologists have been slower to change their views than the physiologists who gave us the concept.

if the animal's behavior as manifested in its learning is dominated by the field properties of the situation, changing with any change in the field, then we shall have to supplement or discard the assumptions underlying the prevailing atomistic theories.[3]

The experiments described in this article were designed to test the learning theories of Thorndike and Watson and at the same time to discover if the white rat, like the ape, shows any evidence of what Köhler has called 'insight.' Specifically, we shall try to answer the following questions: (1) can the rat learn to respond to a *visual structure;* (2) is the rat's response conditioned by or to the *element* connected with the food or other goal, or does the rat carry away something general from a specific learning problem and what is it; does the rat show 'insight' or is its learning merely a matter of conditioning part-responses by chance, pleasure or what not, to form a total response which is adequate to the situation? By 'visual structure' we mean any pattern of stimuli which can be observed by anybody and can be named in such a way as to be communicable to others. 'Insight' may be defined in a number of ways and may be tested through a number of criteria, such as ability to respond to a part in the light of the whole, modification of activities to meet the exigencies of a situation in a manner we may call sensible, or the transposition of the general properties from one situation to another; but in no case do we mean by insight any mental process, a peculiar vitalistic entelechy or anything not objectively observable.

Two sets of experiments under unlike conditions were made with four rats. Since the second set is much more clear-cut, both in setting and results, it will be discussed first.

A box was built to measure 55 x 12 x 11 inches and painted black inside (Fig. 1). The rat was put into the end compartment (10 inches long) which opened into the

[3] By atomism we mean that simple type of theorizing which starts with some simple unit and simply adds such units in summative fashion to explain complexity. The atomism of physics and chemistry has had a different history from psychological atomism, yet the former has been misunderstood by most psychologists from Wundt onwards.

main compartment by means of a door (*E*). At the end of the main compartment were two boxes (*F* and *F'*) the lower third of which was open and illuminated by electric bulbs of various wattage and hence supplying various degrees of illumination. In the first five series of experiments Rats 1 and 2 were given food in the brighter of

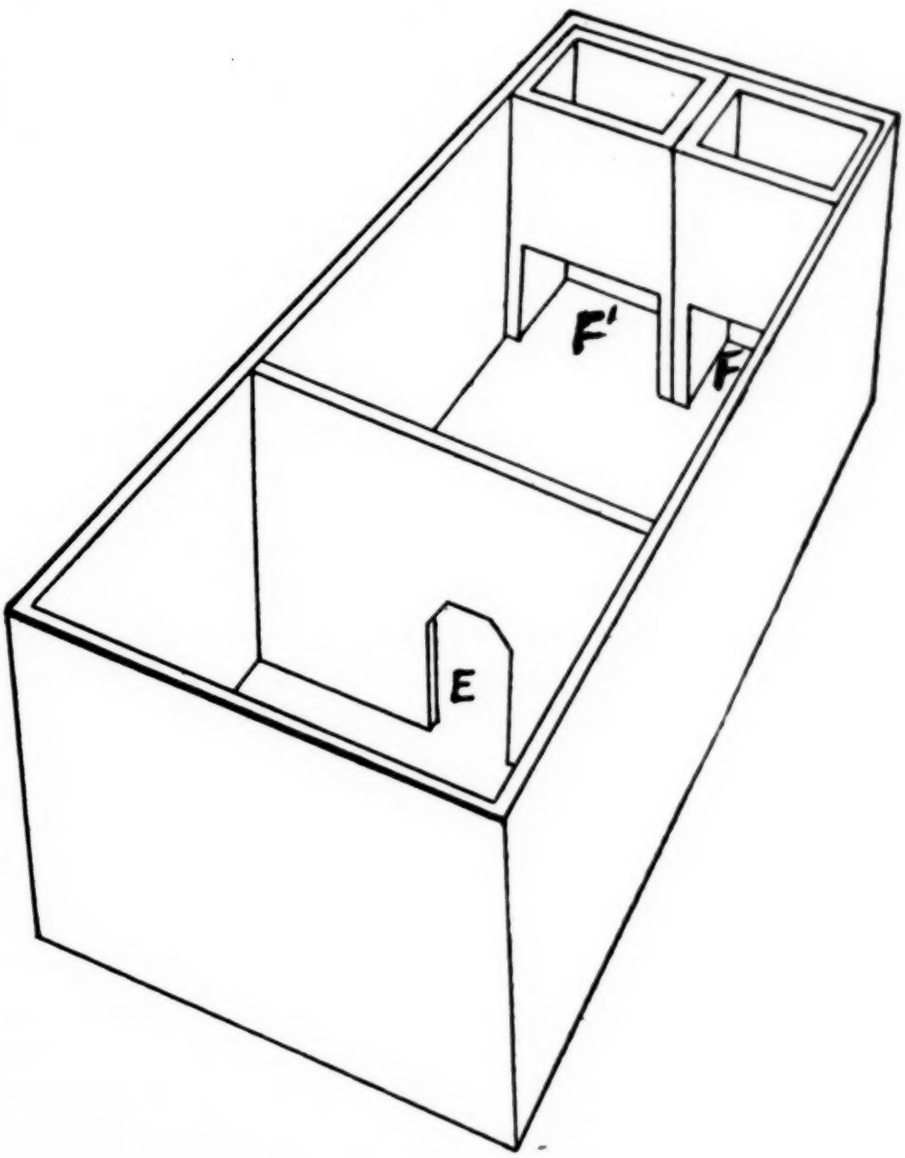

FIG. 1

the two compartments, one illuminated by a Mazda bulb of 60 watts and the other by a small carbon lamp of 2 cp. Rats 3 and 4 were given food in the darker compartment. The rats were not fed except during these experiments and food was given only after the rat entered the correct box.[4] Food was placed in both boxes, of course, to equalize

[4] As we shall see later, punishment may attract the animal. It is well, therefore, under these conditions to make as little commotion as possible at the wrong side, otherwise the animal wastes much time and effort at the negative brightness-box.

visual and olfactory cues and the correct box (the brighter or darker light) was determined beforehand by chance to force a choice on the basis of brightness alone and to prevent the formation of place habits. Small pellets of bread soaked in milk constituted the food reward which the rat could readily grasp and take back to the outer compartment while the lights were being changed, new food being prepared or the boxes cleaned of old particles. The rats very quickly learned to go in the direction of the food-boxes and to try the second box if the first choice was wrong. A correct choice consisted in going at once to the correct box. If a rat went first to the darker (lighter) box when it should have taken the lighter (darker) box, this was counted an error, even though there were several cases where a rat quickly left the wrong box, not touching the food which was right under its nose. Each rat was given at least thirteen and at most twenty-five trials on each day of experimentation. The actual number of trials per day depended upon how hungry a given rat showed itself to be and this was easily determined by the avidity with which it ate the food. When the rat left the morsel in the end compartment without eating it, the experiment was discontinued after three or four such performances. It must be noted, however, that many times the rats went directly to the correct box and still did not eat the food, showing correct responses were made even after the animal had ceased to be hungry.

When first put into the box there was more or less aimless movement on the part of the animal which might better be called exploratory.[5] But after a few trials the rats soon learned to go directly to the lighted compartments where food was to be had on either the one side or the other. Figure 2 shows the average curve of learning for the four rats. The dotted lines in Fig. 2 indicate that one of the stimulus pair was changed so that when the rat was previously reacting to the 60-watt light as the brighter (rejecting the 2-cp light) it was now presented with a new stimulus by whose side it was darker.[6] Thus in Series i–v the stimuli were 60-watt and

We placed the food in a small tin plate which could be hoisted out of the animal's reach if it entered the wrong box. When we used a stick to drive it away from the wrong box the animal at times persisted at it for some time. By our method the rat immediately went to the other box.

[5] Apparently aimless movements may be exploratory or ultimately useful. In one setting (not described in this article) one rat after wandering into a *cul de sac* reached the food but turned about and returned to its place of privacy with a large piece of bread where it could eat undisturbed. It had been the practice of the experimenter to interrupt the rats during their eating in order to start them on another trial.

[6] We are thus repeating with rats Köhler's experiments with the apes on transpositions. See W. Köhler, *The mentality of apes*, 1925. We decided before the ninth series to give just ten trials, taking the percentage of correct performances. Subsequent experiments justified our stopping here. The performance varied but little from the level reached at this point.

2-cp lights; while in Series vi–viii they were either 150- and 60-watt bulbs (for the rats reacting to the brighter light) or 2-cp and 1-cp (shaded almost to darkness) lights. In Series ix all manner of combinations were employed, as 150- and 2-cp, 2-cp and 100-watt lights. The percentage of

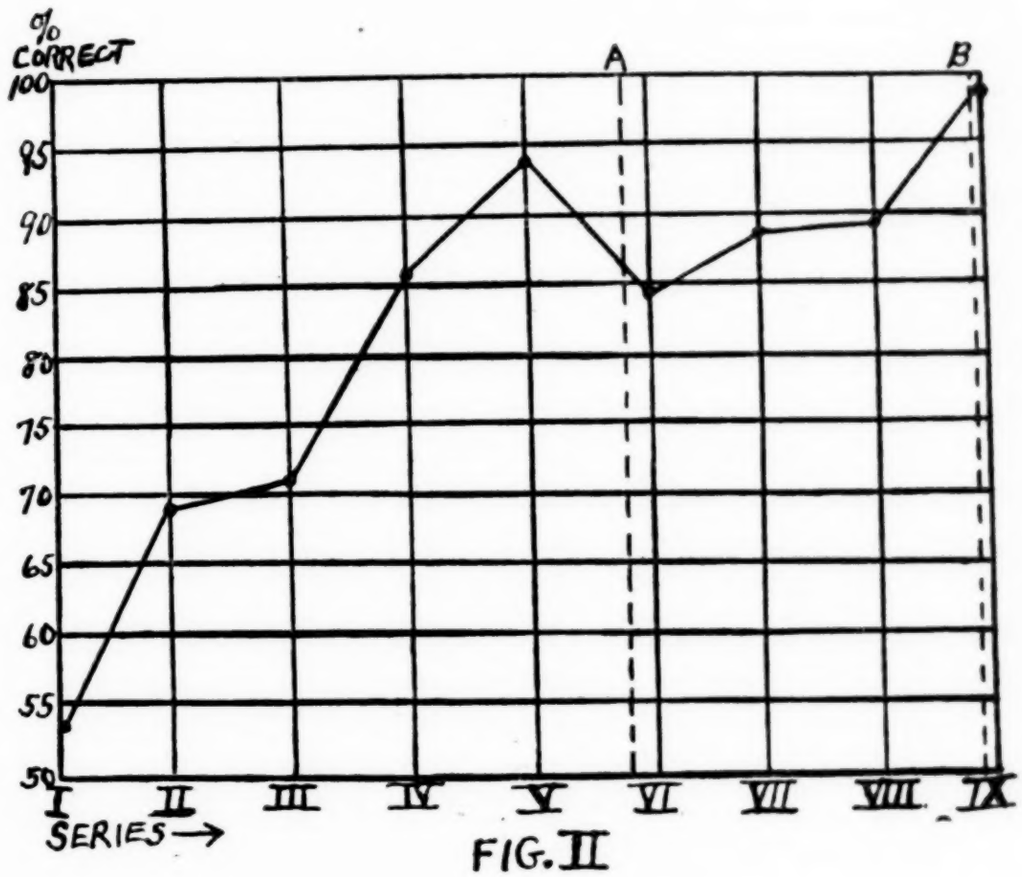

FIG. II

correct responses is based on first reactions to the *lighter* or *darker* stimulus, according to the previous training of the rat. The curve shows that the rat responds to and retains the structure, neglecting the absolute element as such; *e.g.*, although Rats 1 and 2 had been choosing the 60-watt light in the first five series of experiments, when presented with the 150-watt light they chose the brighter and neglected the 60-watt light as they had previously been neglecting the 2-cp light.

The curve (Fig. 2) shows a continuous increase in the number of correct responses from the first day until the sixth day when there is a slight drop. That the drop at this point is not due to the new stimuli appears from the fact that there is no drop in the curve on the ninth day when new stimuli were again substituted. It should also

be noted that two weeks elapsed between the third and fourth series of experiments yet there is a decided increase in the percentage of correct responses. Since the stimuli were arranged by chance and there were only two possible choices, we should expect the rats to get fifty per cent. correct by chance at the beginning of the experiments,—which is about what we find. But the curve does not do justice to the best performances; when, *e.g.*, Rat 1 made 18 correct responses consecutively. Another fact of importance is that the rats generally made their choice at the moment of release at *E*, 18 inches away from the food boxes, whence they went in a straight line to the brighter or darker box, according as they had been trained to choose the one or the other.

We now have to examine our results in the light of the theories of Watson and Thorndike and the general assumptions underlying them. Having reacted to the lamp of 60-watts at least 100 times, why was the percentage of responses in favor of the 150-watt light when presented with the old element? Does transfer of training occur when there is an identical element in the old and new situations? Does learning follow the laws of recency, frequency and vividness? If so, then the rats should have picked the old member of the new pair, *viz.*, the 60-watt light (or the 2-cp when it was presented with the shaded 1-cp light). Even though there is a slight drop in the average on the sixth day, probably due to lack of spacing which is more than compensated by the gain on the ninth series, the results are unmistakably against the atomistic theories. All of the rats rejected the elements to which they had responded the most frequently, the most recently, and with the greater amount of pleasure, choosing rather in accordance with the structure-function principle. What the rats learned to respond to and what they carried away from the experiments were structure-functions and not a series of reflexes conditioned by a given element in the situation. It is only from a knowledge of the structure-function that we can predict what the rat will respond to and not from a knowledge of the elements. Since the structure is common and observable to all, we are not forced to the doubtful expedient of stretching the concepts of conditioned-reflex and neural pathways further than seems safe at present.

A student (C. F. Seitz) working independently but under the same conditions as the writer confirmed the facts described above. The performance of one of his rats

I record here in his words. "Rat 1 made the following percentage of correct responses from January 5 to January 13: 50, 74, 94.5, 72, 85, 100, 100, 100. On January 11 new stimuli were first used, yet the rat made 100 per cent. correct responses. On the last day of experimentation, January 13, this rat, whose stimuli had been a 60-watt light (correct) with a 2-cp light (wrong), was presented with a 2-cp and a 1-cp light. This 2-cp light had been found to be wrong by the rat before; but it went to it immediately for ten successive trials. In the 11th and 12th trials with the 60-watt light and 2-cp light the rat chose the 60-watt light. From the 13th to the 20th trials 60- and 150-watt lights were used and the rat chose the 150-watt light each time. Trials 21 to 25 were made with 1- and 2-cp lights and the rat again chose the brighter. Trials 26 to 28 were made with the 150-watt light and the 1-cp light with correct responses each time, making a total of 28 correct responses to the brighter light." The writer's Rat 1 chose the 2-cp illumination when one box was lighted by it and the other left in darkness.[7] This same rat on February 26, six weeks from the time of the last experiments tried, made nine correct responses out of ten.

We have here, as it seems to me, conclusive evidence that the rats can and do react to a brightness-structure which may properly be called a visual field, since the stimuli act upon the visual receptors of the rat and act as a unit upon them. It is to these visual field properties we must look if we are to find the determining factors in the animals' behavior. To assume either that the rat has been conditioned in the manner prescribed by any one of the conditioned-reflex theories, or that the rat retains a memory-image or is governed by 'transitional sensations' is to plunge into the unknown regarding both the neural mechanisms and the mind of the animal. To say that the animal reacts to and retains a structure-function is to go no further than the observed facts and incites us further to vary conditions in order to determine the limits of the animal's intelligence. Moreover, our concept of the structure-function enables us to explain some facts which we must now face and which are not so easily explained by the older theories of animal behavior.

Certain apparent anomalies in these experiments now call

[7] It thus appears that while darkness is different from light, both phenomenally and perhaps physically, the dark box here functioned as a weaker light for the rat. We have here an objective test in favor of Koffka-Cermak and against Hillebrand who argued that if two phenomenal data are different one cannot be used as the limit of the other in any sense. This is the descriptive point of view carried to an extreme. Here we assumed that darkness is a limiting case of less brightness and the behavior of the rat, based on a structure-function or position in a structure, justifies us in the assumption. See the writer's Psychology of *Gestalt*, *Amer. J. Psychol.*, 1925, 36, 510.

for explanation and their explanation will throw some light upon the method by which the correct stimulus is chosen and the wrong one is neglected.

It is on this very point that the theories which are based upon an atomistic view of behavior seem to us to be particularly pregnable. We infer from the theories of Thorndike and Watson that the correct stimulus is chosen through the *elimination* of the act or acts leading to the wrong stimulus. According to Thorndike pleasure stamps in the correct responses, while punishment has the opposite effect on the wrong ones. Watson's theory of the elimination of the wrong responses, based as it is upon a blind set of conditions and an unjustified use of logical probabilities which will occupy us shortly, need not detain us here. Actual observation shows not only that choosing is a positive phenomenon but also that rejecting is just as positive. Indeed, the wrong stimulus, punishment and those parts of the experimental setting which form part of the punishment complex, may have as decided and positive effects as the reward of food and escape from the unpleasant. We must therefore account in a positive way for the neglect or negative response to the wrong stimulus and the correct positive response to the stimulus which should elicit a negative response according to the older theories. In the first place, we must give up the 'either-or' implication that goes with the Thorndike and Watson assumptions, because (1) the rat is not conditioned to particular elements in the situation, and (2) it does not "*either* go one way *or* the other," thus learning one light by going more frequently to it or experiencing more pleasantness with one stimulus than with another. The results show that neither frequency, recency nor pleasantness conditions a response to the element which has all of these in its favor.

Now for the actual behavior of the rat. It has been stated that after a rat learned to respond to the brightness it made a straight path to the correct place. If it did not go directly to the correct box, it was obliged to turn about and describe a half circle in order to get to the other box. Let us take an extreme example of learning to respond to the correct brightness box and see what actually happens. A glance at Fig. 3 shows the actual path described by an animal which had formed a place-habit and persisted in going to the left, regardless of the position of the lights.

In order to break this left place-habit the correct light was placed on the right. Path number 1 in Fig. 3 shows the animal going directly to the left. How did the animal learn to go to the right, and, finally, to react to brightness? It was not by eliminating the left path, as we should expect from the Thorndike-Watson 'either-or' theories, because the second path taken by the animal was actually first toward the left,

then a sudden turn to the right. The next time (path 3) the path was simpler and the fourth time it was becoming still simpler, until at the sixth trial the rat made a bee line for the correct light on the right. What we have, when conditions permit, is a simplification of motor configuration and geometric

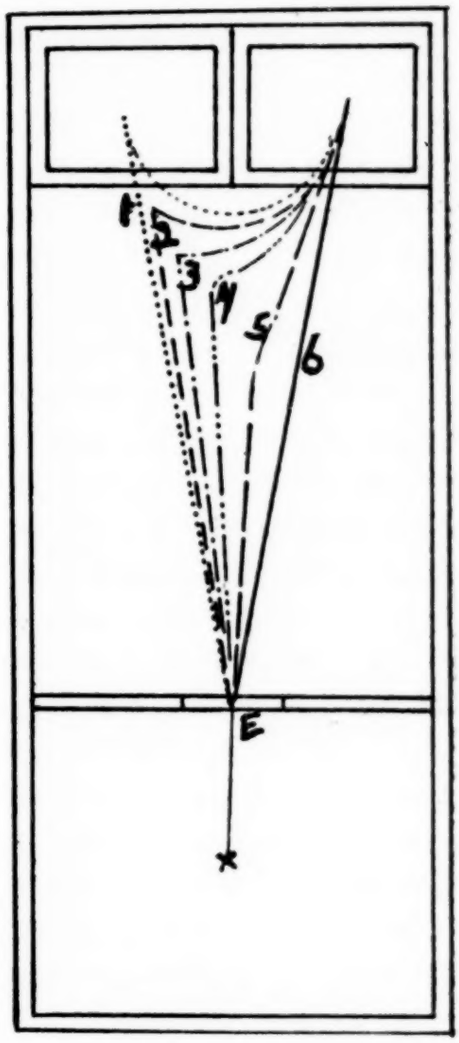

FIG. 3.

path in the direction of the goal. The fact that the animal does not make a straight line for the right until the sixth trial shows that the left still has some attraction for it which the 'either-or' theories neglect when they describe learning merely in terms of positive or negative responses.

Let us follow this rat's behavior a little further. After the sixth trial the correct light was put on the left. The rat

made a straight path directly to the left! In this trial both the remnant of the left place-habit and the factors favoring a response to brightness (this rat had to choose the brighter) are all on the left side and we should expect what was found, even though the rat had made five consecutive responses to the right. The correct light in the seventh trial was then put on the right and we must now ask, what kind of path did the rat make and where did it go? On the basis of the 'either-or' theories we may make a fifty-fifty guess; but neither guess will give us the facts, for we wish to know what the rat actually did. As a matter of fact the path described in the seventh trial was almost identical with that in the fourth trial as shown in Fig. 3. The rat had not entirely freed itself from its place-habit but it was learning to respond to brightness—the actual curve gives us a possible measure of the strength of place-habit *vs.* brightness-response. Taking the total situation into account and the positive value both sides may have for the animal, the rat will not make a straight path when some factors favor the left and others favor the right side. We may say, to put our facts into a convenient pictorial summary, that the actual path taken by the animal represents the least energy path to the goal, provided the positive effect of the wrong side is taken into consideration.[8] We are thus in a position to explain the actual behavior of the rat by taking into consideration the structure-function character of the stimulus and the response called forth by it. If the goal and the means for its attainment are featureless, the path described by the animal in reacting to them will be the more complex the greater the obscurity surrounding the problem. Conversely, when the goal is so determined by

[8] The reader will immediately think of Rowan Hamilton's principle of least action which has been called the most universal principle of mechanics. While there is a faint resemblance between the shortest path chosen by a hungry animal to food in sight and some of the facts covered by Hamilton's principle, we do not mean to imply that the principle of least action, pure and simple, is here operative. Animals often waste time and energy, even when hungry and the food is in sight, for reasons at present unknown. At any rate the principle is suggestive of further experiments. A shortening of path has been reported by G. D. Higginson in his Visual perception in the white rat, this *Journal*, 1926, 9, 337 ff.

the structure that the path to it appears from this structure, then the reaction-pattern will be the simplest in accordance with our principle of least energy. Or we may say that the responses necessary to reach a goal will be a minimum whenever the field properties of the total situation are sufficiently differentiated to elicit a structure-function response. If for any reason the stimulus-complex is not differentiated we cannot expect a differential response. We can thus understand how the wrong stimulus and even punishment may exert a positive effect upon behavior and why the modifications in behavior in these experiments consisted not in choosing either the right or the left box, stamping in the one and stamping out the other, but rather in a gradual simplification in response from the time of leaving the end compartment to the attainment of the food.[9]

We are now in a position to consider the more complicated behavior exhibiting insight in the set of experiments which we first performed. In the second set (described above) we have used the ability to transpose structures as a criterion of insight. We must not infer, however, that all cases of transposition spring from insight; in fact, we wish to point out that even though the rat describe a straight path from the point of release to the correct light, it may do so, after the problem has been learned, quite mechanically. Rats are impulsive, liable to action and apparently more given to movement than to surveillance. But even though learning may degenerate into blind mechanism it does not follow that it arose in that way. We must not forget that an apparently mechanical response may be preceded by insight which determines behavior in a manner wholly different from that postulated by the atomistic theories of conditioned-reflex, trial and error,

[9] Snoddy has shown that the 'either-or' view of muscular action does not suffice to explain the tracing of a star-pattern by its mirror image. In his experiments learning did not consist in stamping out the incorrect responses nor did it occur where the most pleasure was experienced, but rather learning occurred at the places of greatest difficulty and discomfort and consisted in a simultaneous fusion of opposed muscular systems. This paper by Snoddy seems to the writer to be one of the most important published in recent years. See G. S. Snoddy, An experimental analysis of a case of trial and error learning in the human subject, *Psychol. Monog.*, 1920, 28 (no. 124).

and recency-frequency. In the making of sensible adjustments, in modifying behavior to meet the exigencies of a situation, in adapting part movements to fit into a behavior pattern adequate to a problem we find that habitual responses may need to be entirely discarded or re-ordered. Such activity we may accept as a criterion of insight.

The box described above was used again in these experiments, with certain modifications which appear in Fig. 4.

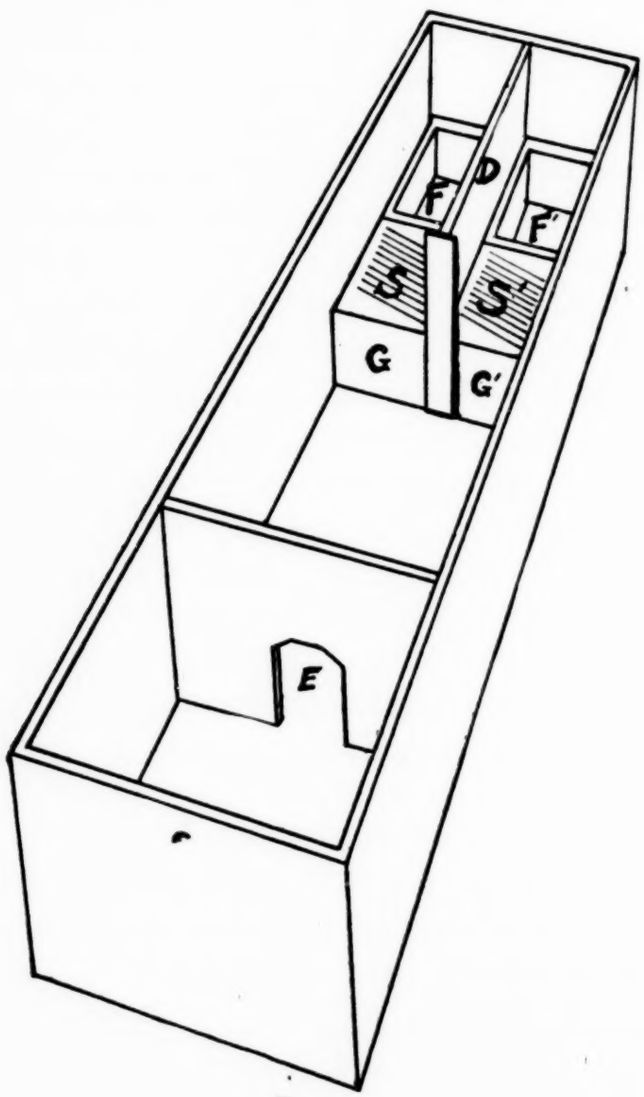

FIG. 4

The rat was placed in the end compartment and allowed to pass through the door *E* into the next compartment where it was faced by two gray papers, *G* and *G'*, 5½ x 4½ inches. *S* and *S'* were alleys or platforms wired with annunciator wire so that a current from an inductorium could be turned on the wrong side to serve as punishment

for responding to the wrong gray. *F* and *F'* contained food, the wrong box being covered with wire which prevented the rat from eating if it reached it. *D* was the central partition dividing the two alleys and food boxes. The rats, it was supposed, could be trained to respond to either darker or lighter gray, climb upon the platform and go to the food box at the rear. If a wrong choice was made, then punishment was administered, and if the rat got as far as the food box it could not get at the food. The supposition made here was that if a rat was punished every time it chose the lighter gray and fed when it chose the darker it would finally eliminate the lighter and respond only to the darker. We tried to train two of the rats to choose the darker gray and two the lighter, as in the experiments described above. Milton-Bradley grays were used, numbers 10 and 30 being deemed sufficiently unlike to elicit a differential response from the rat. The grays were changed in a chance order.

The results, so far as building up a differential response to the difference in brightness is concerned, were disappointing; but it must be remembered that the conditions were different from those described above where we did get a differential reaction to the lights used as stimuli. But more gratifying and enlightening are the methods developed by two of the rats for reaching the food without punishment. They were entirely novel and hardly expected by the experimenter, who believed that the only 'sensible' solution must consist in a choice of the correct gray to which the animals were being trained. Although the experiments were continued for over twenty days, with at least five trials a day, at the end of over 100 trials none of the rats had learned to respond to the brighter or darker of the two grays. We may account for this fact by the lack of connection between the grays and the food since the wired alley intervened, with punishment, between them. Both Wertheimer and Müller emphasize the importance of nearness in the mutual influence of members of a complex.[10]

On the other hand, the conditions demanded by the Thorndike and Watson theories were satisfied because all four rats accidently chose the correct gray, passed the alley without punishment and reached the food, yet no learning took place. The correct response was neither stamped in nor conditioned. Out of a total of 102 runs Rat 1 should have made, on the basis of the 'either-or' theories, 51 correct

[10] G. E. Müller, *Komplextheorie und Gestalttheorie*, 1923; M. Wertheimer, Untersuchungen zur Lehre von der Gestalt, *Psychol. Forsch.*, 1923, 4, 301.

choices by chance; whereas, as a matter of fact, this rat made 32 correct and 70 wrong choices. The actual records show that instead of getting better, the performance grew worse. Rat 4 became so afraid of the electric shock that it finally gave up the problem, huddling in a corner and refusing to attempt the alleys. This rat would have starved had it not been fed after being returned to its cage. The behavior of Rats 2 and 3 will be described in a moment; but the point to be noted here is that when there is absolutely no insight, *when the conditions of chance are actually imposed upon the animals, no learning at all takes place.* Subsequent pleasure does not retroact to stamp in the correct initial act, nor does the frequency-recency hypothesis account either for learning or for failure to learn. We might expect the animals to avoid the side where it had just received punishment if the older theories are to be taken seriously; but often a rat tried again and again to overcome the bad place, sometimes skipping across the wires quickly, jumping, and the like.

The method developed independently by Rats 2 and 3 for reaching the food without punishment appeared very suddenly and with such great certainty and naturalness as to leave no doubt in the observers as to its nature. Rat 2 utilized the method on the fifth day of experimentation; while Rat 3 did not use it until the ninth day, showing a difference in intelligence as measured by insight. Before describing the method used we must first know what the rats did before developing it. All of the rats quickly learned that when food was not to be had after traversing one alley it was at the end of the other. But to get to the other food-box meant retracing the wired alley and more punishment, and, since even the dead wires were feared as much as the live, both the longer and more complicated path of correcting a wrong choice and additional punishment conspired against merely going to the other food-box. We have evidence of a spread from purely tactile stimulation to vision; for after two or three series of experiments the mere sight of the wires elicited the same recoiling reactions which the shock

originally called forth. We should therefore expect from our principle of least path and the to-be-avoided character of the wired alleys that the rats would shorten their path to the food and steer clear of the wires if possible. This Rat 2 did on the fifth trial of the second series of experiments. Instead of retracing its way back across the wired alley after making a wrong choice, as it had done nine times before, it climbed directly across the partition into the food-box on the other side (*F* to *F'* in Fig. 4). That this was not due to the odor of the food on the other side seems certain because the food lacked any appreciable odor. So far as we could discover in these experiments, the rats never were guided by odor, and, moreover, the food directly under the rat must have been very much more odorous than that on the other side. At any rate, we may leave this point because this method was never used again before the final method we must now describe. In the second trial of the fifth series of experiments, Rat 2 approached the grays, mounted the alleyway on the left so as to avoid the wires, climbed to the top of the partition, and ran directly to the rear until it was directly over the food-boxes and then descended into the food-box not covered with wire, the last possibly by chance. Rat 3, without ever having even climbed over the partition from *F* to *F'*, repeated exactly the performance of Rat 2, its successful solution coming in the ninth series of experiments on the 36th trial. This method had never been used by either rat and, unless one indulges in the greatest stretch of imagination, it cannot be explained by the process of conditioning so popular in present-day experimental literature. The rats had taken the shortest possible route to the food in avoiding the wires and in doing so had adopted a method so novel and sensible that both the experimenter and others who watched the animals were taken by complete surprise. Rat 2 used this method 16 consecutive times with intervals ranging from 24 to 48 hours between successive series of experiments and until prevented from climbing by a wire netting placed on the top of the box. When the wire netting covered only the forward portion of the box, to force the rat to choose the gray after

getting into the wrong box, instead of going back over the wires, it then took the shortest cut to the food by climbing over the partition as reported above. When the wire netting was removed this rat resorted to its old method of avoiding the wires!

Rat 3 was more labile in its responses than Rat 2. It climbed from either the right or left to the top of the partition; but Rat 2 always approached and climbed from the left. Neither rat, with one exception, ever descended upon the wires. It is probable that if Rat 1 had been bigger it might have developed the method of the other two rats because it made attempts at climbing the walls of the box (not the partition) which were generally unsuccessful. Once when it did succeed in jumping to the top, it ran around in aimless fashion, apparently wholly disoriented. It was clever in pussy-footing across the wires, hugging the sides of the alleys and in a general cautiousness when near the wires.

We have now to ask if the rat had a memory image of the food in the rear boxes when it first climbed upon the central partition and when it chose the brighter (darker) of the new stimuli. This question concerns the mechanism by which the first correct response is made and also carried over from one series of experiments to another. If the answer is in favor of the memory image we should have to believe that a rat can reinstate a situation not only after two days but even after six weeks. Apart from the great uncertainty which must attend any speculation on the mental processes of animals, it hardly seems probable that memory images either are or need be used after such long intervals. Yet the time given by Hunter for the delayed reaction of the rat is often taken as a measure of the rat's ability to carry over a successful response from one time to another, and even as a measure of its ability to retain 'ideas.' Interpreted in this way, 1 to 5 seconds hardly seems to be sufficient. We must remember, too, that in Hunter's experiments the situation was different from ours and probably more difficult, inasmuch as the rat had to respond to an absolute element in an absolute position. Since we know from the reports of human subjects that any or all of the memory processes may run their course without the mediation of imagery, it is unnecessary, indeed wildly speculative, to assert that, even in cases of insight, images, ideas or other 'mental' processes need be present.

Wertheimer and Köhler have laid the foundations for a physiological theory designed to account for configurational responses and insight without the assumption of sensations, images, or even activities previously utilized. Stated briefly, we assume that physiological processes must be regarded as total, structured events whose properties are determined by

the field properties of the stimulus-pattern. Reflex is not added to reflex through frequent repetition, but rather the specific form of an environing situation determines the form of the response and thus the part-activities. Instead then of saying that a memory image of the wires and food actuated the rat to modify its behavior in a sensible manner (a possible explanation but hardly verifiable since we cannot control memory images) we prefer to say that the food at the end of the path modifies the character of the approach, investing it with an entirely different significance so that given a structure in which each part influences every other, the presence of one part elicits a response appropriate to the whole. The initial, transitional and final (or consummatory) responses form part of a single, dynamic act adequate *in parte et in toto* to the problem confronting the animal. While we realize that such a description does not constitute a complete explanation, it does raise a host of interesting problems that may prove fruitful in future animal experimentation. Subsidiary hypotheses like our principle of least energy follow from a configurational view of the stimulus-situation and the resulting responses and suggest definite experimental problems in settings which may give us more information about the nature of intelligence, the mechanisms of learning, and the limits of ability.

Conclusions

From the conditions and results of our experiments we feel justified in concluding that

(1) The rat can and does react to a purely visual structure;

(2) Transfer of training takes place in accordance with structure-function principles and not in accordance with the theories of identical elements, recency, frequency and vividness;

(3) When the conditions of chance obtain, no learning takes place;

(4) Insight, as we have defined it, appears in the behavior of the white rat and cannot be explained away by any theory

which begins with the assumption of unit activities meaningless in themselves and joined together without significance,

(5) A proper approach to the problems of animal psychology can be made by means of useful hypotheses which do not do violence to the facts and yet are suggestive of further work and capable of observation, verification and measurement.

REASONS USED IN SOLVING PROBLEMS

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The present paper is an attempt to study the reasons reported by individuals engaged in problem-solving. The study grew out of an experiment, the general plan of which was to confront individuals of different ages with problems objectively as similar as possible and to note resemblances and differences in their procedures. It soon became apparent that one of the most marked differences between more mature and less mature subjects occurred in the reasons they used while thinking; that children and adults differed both in the *kinds* of reasons they gave and in the *extent* to which they gave reasons at all, and that there were indications of a development from less mature to more mature modes of response.

The reasons were singled out as a special object of study partly because of the circumstances in which they occurred, *i.e.* in a setting of actual problem-solving and presumably when they were actively functioning in an adjustment process, and partly because the trend of the evidence is quite in line with that of other experimental studies of the thought activities of children and adults. Such studies have indicated, with considerable consistency, that particular age-levels are likely to be characterized by particular modes of response. In a sense, the intelligence tests of the Binet type may be regarded as a demonstration of this fact, since the very possibility of measuring an individual in terms of mental age implies that given responses will occur at given ages with enough regularity to justify prediction. It is especially significant that, in some of these tests, highly similar tasks bring out very different responses at different ages; that words are defined, *e.g.*, first in terms of use and then in terms superior to use; that a picture is commented on first by a mere enumeration of the objects portrayed, later by description, and still later by interpretation.[1]

Studies in association, too, have shown that various responses may be expected at the various age-levels. Rosanoff and Rosanoff[2] report that *common* responses are not so frequent among children as among adults, but that, as age increases, the responses of children grow more similar to those of adults until, at about eleven, the difference

[1] L. M. Terman, The measurement of intelligence, Houghton Mifflin, 1916. See esp. pp. 167 ff, 221 ff; also pp. 145, 190 ff, 302 ff.

[2] I. R. Rosanoff & A. J. Rosanoff, A study of association of children, *Psychol. Rev.*, 1913, 22, 43–89.

between the two groups practically disappears. Woodrow and Lowell,[3] who made frequency tables for children, using the Kent-Rosanoff list of words, found not only that there were common responses for children, which differed from the common responses of adults, but also that certain *forms* of association appeared more frequently in one group than in the other, *e.g.*, that contrast and coördination were more characteristic of adults than of children and that children were more likely than adults to use verb-object and noun-adjective responses. Thus in free association, as in the reasons reported in this study, there are indications both of characteristic age-differences and of a development with increasing years.

The term *reason*, as it is used in the present paper, applies to any indication, either by word or by gesture, of the basis on which the subject was reacting. Anything was classified as a reason which the subject offered as such, without reference to any standard of logical acceptability. Furthermore, the reasons were treated simply as reactions made by individuals during problem-solving. No attempt was made to analyze them beyond this point or to determine their possible further significance.

The Experiment

As has been indicated, the general plan of the experiment was to place children and adults in problematical situations objectively as similar as possible and to record similarities and differences in their behavior. In particular, an attempt was made to eliminate the factor of information; to arrange conditions which would give adults as little advantage as possible because of superior knowledge and which would thus make the differences that appeared as largely as possible a matter of differences in the procedures employed.

Subjects.—Four groups of subjects were used.

1. Ten children between the ages of two-and-a-half and three-and-a-half years enrolled in the Nursery School of the Institute for Child Welfare, University of Minnesota, during the summer of 1926.[4] These children are referred to as the three-year-old group throughout this paper.

2. Ten children between the ages of four and five years enrolled in the same school at the same time. These children are referred to as the four-year-old group.

[3] H. Woodrow & F. Lowell, Children's association frequency tables, *Psychol. Monog.*, 1916, 22 (no. 97).

[4] The writer wishes to acknowledge the kindness and coöperation of Dr. J. E. Anderson, Dr. Florence Goodenough, and the teachers of the Nursery School in permitting the children to be used as subjects and in making it possible to arrange the conditions necessary for conducting the experiment.

3. Ten school children, from six to ten years of age inclusive, who acted as subjects during the summer vacation.

4. Ten adults, students in undergraduate courses in Psychology in the University of Minnesota during the summer quarter, 1926.

Problems Presented and Manner of Presentation.—Each subject was studied individually and each one attempted three problems. For the children under five, the task in each problem was to discover in which of two closed boxes a doll was placed. The boxes, arranged in a particular manner, were placed before the child, who was asked to open the one in which he thought the doll was hidden. He would thus discover whether he had taken the right or wrong box. Each problem consisted of a succession of such situations in the course of which the child might discover that there was some constant feature which would serve as a clue to the box where the doll could be found. The children were not told that this was the case, but were left to discover the fact for themselves. They were simply told to find the doll. In some problems the clue was the position of the box, in others the design on the cover.

The materials used in the experiment were a small celluloid doll, eighteen small pasteboard boxes which could be opened and closed, cotton which was placed inside the boxes so that the doll could not be shaken around inside and thus give a clue to its presence, a screen behind which the boxes could be arranged, and a stop watch. On six of the boxes were plain conventional figures in black ink: a circle, a diamond, a triangle, a clover, a star and a cross. On six other boxes, the same figures were used, but in the center of each was a flower drawn in red ink. On the remaining six the same figures again appeared, but red dots were placed at irregular intervals around their perimeters.

In each problem the boxes were presented, two at a time, in a particular arrangement. The arrangement, with specified modifications, was kept throughout the problem; but the boxes were changed in each presentation. The several problems were as follows.

Problem I. The boxes with plain conventional figures were used. Two boxes were arranged, corner to corner, one nearer and the other farther away. The doll was placed in the nearer, right-hand box. This situation was repeated,—*i.e.* the arrangement was the same, but different boxes were used each time,—until the child reacted correctly five times in succession.

The situation was then changed so that the left-hand box was nearer the child, the right-hand box farther away. The doll was again placed in the right-hand box. This situation, too, was presented until five correct reactions had been made in succession.

The two arrangements were then presented in random order until the child had

reacted correctly ten times in succession. The problem was considered solved if the child was able to do this. The solution consisted in taking the right-hand box, regardless of whether it was the nearer or the farther one.

Problem II. Two kinds of boxes were used, those with plain figures and those with red flowers in the centers of the figures.

Two boxes, one plain and one flowered, were presented side by side. The doll was always placed in the flowered box. This box was placed to the right of the plain box until the child had made five correct successive reactions; then to the left, again until five correct successive reactions had been made; and finally the two arrangements were given in random order until the child had solved the problem—*i.e.*, had reacted correctly ten times in succession, always taking the flowered box regardless of position.

Problem III. The boxes with the plain figures were used, and also those on which red dots were placed on the perimeters of the figures.

In the first presentations, two boxes with plain figures were used, one immediately behind the other. The doll was always placed in the farther box. After five correct successive reactions, the boxes with dotted figures were used. The arrangement was the same, but the doll was placed in the nearer box. This situation was continued until five correct successive reactions had been made. The two kinds of situation were then presented in random order until ten correct reactions were made in succession, the child always taking the farther box when the plain figures were used, the nearer one when the dotted figures were used.

Details of the Procedure. As a preliminary to the experiment proper, the child was seated at a table, shown the doll and two of the boxes, and permitted to examine and handle them. He was told that the doll was to be hidden in one of the boxes and that he was to find her. A screen was then placed on the table between the subject and the experimenter, and the two boxes, one containing the doll, were arranged behind the screen. (The arrangement was not the same as that used in any of the problems in the experiment proper.) After asking "Are you ready?" the experimenter drew aside the screen and asked the child to find the doll. The reaction time—*i.e.*, the time between the exposure of the boxes and the child's selection of one of them—was taken by means of a stop watch. In case the child opened the wrong box first, he was permitted or encouraged to open the other one and find the doll. In either case, he was asked to tell why he thought the doll was in the first box selected.

The question was put in three forms, "Why did you take that one?" "Why did you think she would be there?" or "How did you know where to look for her?"; but in order to keep the conditions from seeming too set, the question was introduced by such comments as "You found her again" or "You found her that time, didn't you?" or "I wonder why you didn't find her that time." An attempt was made to keep the situation as informal as possible, to put the child at ease, and to encourage him to talk.

After the child had reacted to the first situation, the doll and boxes were withdrawn, the screen replaced, and the procedure repeated. Three or four reactions of this sort were taken to habituate the child to the conditions.

The experiment proper began when Problems I, II, and III were given in the order named and according to the procedure described. In order to keep the children from getting tired or bored, the problems were presented on three different days; *i.e.* not more than one problem was attempted at a single sitting. The children were usually not kept more than twenty minutes, but occasionally they remained as long as half an hour.

The Record.—The record, taken on blanks prepared in advance, gave the following information:[5]

The temporal sequence of the reactions
The correctness or incorrectness of each reaction
The reaction time of each reaction
The reason for the reaction as stated by the child, or a note of his failure to give a reason
Brief notes on the comments made by the child, or anything else in his behavior which the experimenter thought might be significant

Immediately after the close of each problem, the experimenter wrote a brief description of the child's behavior on that occasion. This description was included as part of the record.

Modifications in Procedure for Adults and Older Children.—Three modifications in procedure were made for the two older groups.

1. The doll was not used. The subjects were told that one box contained cotton, that the other was empty, and that they were to discover in which box the cotton was placed. The reasons for not using the doll are obvious in the case of adults. In some preliminary experiments with children from six to ten, it was found that the presence of the doll made some of them feel that the experiment was intended primarily for younger children and somewhat childish for their years. Besides, after the first few reactions, the adults and older children did not, as a rule, open the boxes to see whether they were right or wrong. They simply indicated one of the boxes, stated their reasons, and waited to be told whether they had made the correct reaction.

2. The instructions were more formal. After the subject had made his reaction, he was asked, "Will you tell me why you took that one?" This question was repeated when necessary, but after the first few reactions, the subjects usually gave their reasons as a matter of course.

3. The older children and adults solved all three of the problems at one sitting.

In other respects the procedure was the same as that described for the younger children.

Treatment of Results

The data were analyzed quantitatively from two points of view.

First, the four groups were compared with respect to the *frequency* with which they gave reasons for their actions. This was done by determining for each group and for each problem the number of reasons in relation to the number of

[5] Not all the information referred to was utilized in the present study which takes up only one aspect of problem-solving. It is mentioned because the recording of the information was a part of the regular procedure.

reactions per problem. These data are presented in Table I, which shows (1) the number of reactions made by each group to each problem; (2) the number of reasons given by each group in each problem, and (3) the ratio of the number of reasons to the number of reactions or the number of reasons per reaction.

Secondly, the groups were compared with respect to the *kinds* of reasons employed. This involved classifying the reasons and determining for each group and each problem the percentage of reasons occurring in each class, using the total number of reasons given by each group in each problem as the base. The results of this analysis are given in Table II (p. 408). In this table the reasons are first considered from the standpoint of the age-groups in which they occurred, without regard to the situations that elicited them. The question is asked: Out of all the reasons given by a particular group, what percentage occurred in each class? The table then gives a similar analysis, but takes into account the particular problems involved. Here the question is: Out of all the reasons given by each group in a given problem, what percentage occurred in each class? In neither section is the number of reactions involved taken into account. The general question is: Out of all the reasons given, how are they distributed among the various classes?

The terms 'number of reactions per problem' and 'number of reasons per problem' both require some explanation.

In determining the number of reactions per problem, all reactions were excluded which were mere repetitions of former reactions, due to the requirements of the experimental conditions. It will be remembered that, at some stages of the problem, five correct successive reactions were secured to one kind of situation before the next was presented, and that at the end of the problem ten correct successive reactions were made before the problem was considered solved. These formal repetitions were excluded in order to reduce all the reactions to the same kind;—that is, to those made while the subject was in ignorance of the correct response and while he was still actively engaged in solving the problem. By this device, too, solved and unsolved problems [6] were made comparable in that they all involved reactions to situations to which the subject did not as yet know the correct response.

[6] All of the problems were solved by all the adult subjects and by all the children from six to ten. In the four-year-old group, six children solved Problem I, ten solved Problem II, and one solved Problem III. In the three-year-old group, one child solved Problem I, two solved Problem II, and none solved Problem III.

Similarly, in determining the number of reasons per problem, those reasons were excluded which were mere repetitions due to the experimental conditions. The same conditions which required the repetition of certain reactions would also necessitate the repetition of the reasons that accompanied them and would thus artificially weight the reasons required by the solution in comparison with those elicited by the situation but not fixed by the solution.

Results

I. *Relative Frequency of Reasons at Different Ages.*—The first general fact brought out by this study is that the response of giving reasons is much more readily elicited from adults and older children than from children under five. There are three main lines of evidence on this point.

Table I

Table Showing Frequency of Reasons for Each Problem and Each Group

Age-Groups	No. Subj.	Problem I			Problem II			Problem III		
		No. Reactions	No. Reasons	Reasons per React'n	No. React'ns	No. Reasons	Reasons per React'n	No. React'ns	No. Reasons	Reasons per React'n
3-yr.-olds.....	10	219	5	.02	230	9	.04	216	5	.02
4-yr.-olds.....	10	178	17	.09	23	21	.91	348	10	.03
6–10-yr.-olds...	10	174	231	1.32	23	28	1.22	81	117	1.44
Adults.....	10	34	48	1.41	16	22	1.38	40	64	1.60

1. There is a marked difference in the frequency with which reasons occurred in the responses of various age-groups. The dividing line comes between the ages of five and six. As Table I shows, adults and older children accompanied their reactions with reasons far more often than did the children of the three- and four-year-old groups. The difference is clearly indicated in spite of the small number of cases in each group.

But the meaning of the difference is not so clear. It may be and probably is due in part to differences in linguistic ability. But the reasons referred to in this experiment were not necessarily verbal; gestures which indicated the basis of reaction, and words or phrases supplemented by gestures, were regularly accepted as reasons. Besides, most of the children conversed about other matters and some of those in the four-year-old group would be considered rather talkative.[7] It is significant that it was in response to requests

[7] The children in the Nursery School have been used as subjects in a number of experiments and are rather free from shyness and self-consciousness in those circumstances.

for reasons that failure to respond verbally occurred. This in turn suggests that the difference may be due to an inability on the part of the younger children to understand the instructions. But the fact that reasons were sometimes given in response to the same questions which at other times failed to occasion them, and that the same children who gave reasons in some cases failed to do so in others, indicates that a possible inability to understand the instructions does not wholly account for the situation. Besides, a less clear understanding of the instructions is itself an indication of a different reaction to a situation involving reasons, and implies a greater difficulty in touching off the response in some individuals than in others. The inability to understand instructions is not wholly unrelated to the presence in the individual of the response to which the instructions refer.

On the whole a difference of some kind is plainly indicated, though the reasons for the difference are not apparent in this experiment. It may be said, however, that, in the conditions described, the response of giving reasons during the solution of problems was much more readily elicited from adults and children over six than from children under five.

2. A second point indicated by Table I is that the response of giving reasons was influenced by the objective situation as well as by the age of the responding organism. The clearest illustration of this fact is found in the responses of the four-year-old children to Problem II. Here the number of reasons per reaction was almost the same as for adults and older children. Indeed, in this and in other respects, four-year-old children gave an almost adult performance in this problem. All of them solved it, apparently without difficulty. Evidently the red flower, which was the clue to the situation, was a fairly obvious perceptual feature, easily and definitely identified. This suggests not only that the occurrence of reasons for action is in part a function of the objective situation, but that a situation which is well within the limits of the children's abilities, from the standpoint of solution, is also one which calls out the response of giving reasons.

This does not mean, however, that the solution of a

problem was invariably accompanied by a statement or a gesture indicating a reason. Problem I was solved by six of the four-year-old group; but two of the individuals who solved it gave no reasons for their actions. Only one child in the four-year-old group solved Problem III, but though he gave two reasons during the course of the problem he did not state or otherwise indicate the one that constituted the solution. In the youngest group, most of the children failed to solve any of the problems. One solved Problem I, but did not give a reason; two solved Problem II and both gave reasons. In other words, the data indicate not that reasons and solutions invariably accompany each other, but that the occurrence of reasons is in part determined by the objective situation, and they suggest the possible significance of the fact that the situation which was most suitable to the children's abilities from the standpoint of solution, was also the one which most effectively stimulated the reactions involved in giving reasons.

3. A fact which is somewhat indirectly indicated by the same table is that the reasons given by the older group were more complex than those given by young children. It will be observed that in the older group the number of reasons per reaction is usually greater than one. This means that the reason for a single reaction sometimes contained more than one consideration for action. In some cases considerations were in harmony with each other and suggested the same line of action; at other times the opposite was true and the subject stated as possibilities other considerations which might affect the case. This reaction did not appear in children under five.

II. *Kinds of Reason Used at Different Ages.*—The second general fact brought out by this study is that the age-groups differed in the character of the reasons offered.

1. *Reasons based on observations after the action;* that is, reasons based on observations which could not possibly have been made until the subject had made the reaction to be accounted for.

In children under five. "Because I opened it and saw her." "I opened it and there she was." "Because I saw her feet."

2. *Non-specific reasons;* reactions in which no particular feature or aspect of the situation was singled out and associated with the overt reaction.

In children under five. "Because I guessed it." "I just took it." "Because I knew." In older children. "It was just a guess." "I just tried it. No special reason."

3. *Reasons based on preferences or relative values;* reasons in which likes, dislikes, or æsthetic values were made the basis of reaction.

In three- and four-year-old children. "I like it." "Because it is the pretty one and I like the pretty ones." "The pretty ones." "The nice ones." "The cute little boxes." In older children. "Because it was drawn neater." "Because it was more attractive." "It is fancier. There is more to it." "It is more unusual." "The other is a more common design."

4. *Reasons based on particular features of the objective situation;* reactions in which the subject definitely responded to some particular feature, even if there was no 'reason' in the more critical sense of the term, why one feature should be more important than another.

In the younger children. "The ones with the flowers on." "The flower." In older children. "Because it looks like a star." "The one that looks like a watch." "Because it is round like the sun." "This one, because it is like a tent." In adults. "The circle." "The triangle." Sometimes adults explained these reactions, as in "I took the diamond. I think maybe there are certain figures which are always right and we have to find out which ones."

5. *Reasons based on spatial position or relationship;* indications by word or gesture, usually both, that the reaction was based on responses to those factors.

In the four-year-old children. "On this side," with a wave of the hand to the right. "The one up here" or "This one," pointing to one of the boxes. One child replaced the boxes as they had been presented and said, "This one," pointing to the one in the correct position. In the children from six to ten. "The one closest to my hand." "The one nearest to this," pointing to the screen. "This one up here." "When they are like this, you take the nearest, when they are the other way, take the one that's farthest away." In adults. "Those on the right." "The farther ones." "Nearer for the red dots, farther for the others."

6. *Reasons based on past successes or failures;* repetitions or avoidances of former reactions because they had been right or wrong before.

In the four-year-old group. "Because she was there last time." In older children. "Because it was right before." "Because I missed on that one once before." In

adults. "I think that has been right before." "I don't think it has ever been right when I have taken a triangle. That is why I took the other one."

7. *Reasons based on changes in the objective situation;* reaction based on the possibility that a change in the situation might call for a change in the response.

In children from six to ten. "Because it is up here now." "Because it is different." "Oh, wait a minute! It was closest to my hand this way," pointing to the right, "but now it's closest to my hand this way," pointing toward herself. "I should have taken the other one because it is different!" In adults. "I noticed it was different and thought I ought to take the other one." "The dots haven't been there before and I thought they wouldn't be there for nothing, so I changed to the opposite of what has been right."

8. *Social reasons;* reasons based on the fact that the experiment was being operated by another person.

In children from six to ten. "I thought you'd try to fool me and hide it in the other one this time." In adults. "I thought it would be pretty obvious if it were simply the one with the flower and you wouldn't be so likely to use that."

9. *Reasons based on suggested significance;* reactions which took into account possible conventional or symbolic meanings of the drawings presented.

In children from six to ten. "Because a circle stands for friendship. It never ends And the other is a diamond, and friendship is better than diamonds." "The star, because a star is something high, you know." "Well, a four-leaf clover means good luck and this is almost a four-leaf clover." In adults. "The red might mean danger and we'd have to do something entirely different." "I am trying to make some kind of a design of those red dots. I think they are supposed to be arranged so that they will give a clue."

10. *Reasons involving a restatement or revision of a hypothesis found to be correct;* a shortening of the formulation previously found successful, usually taking the form of a more complete generalization, but sometimes of a mere verbal condensation.

In children from six to ten. "Oh, always closest to my right hand. The ones on the right," after the subject had used the formulation "Closest to my hand this way," pointing toward herself, "when they are like this, and closest to my hand this way," pointing to the right, "when they are the other way." In adults, "Those on the right," after the solution had been stated in the form, "If they are slanting this way, take the upper; the other way, the lower." "Dots, lower; plain, upper," in place of "When the plain figures are used, take the farther; otherwise take the nearer."

11. *Reasons based on the occurrence of a test case;* reactions in which a given response was made, not because it would be

right or wrong but because it would isolate and demonstrate a particular fact.

In adults. "Well, as soon as I saw that, I realized that I might have been getting them right either because they were the lower ones or on the right-hand side. I took the lower one, not knowing whether it was right or wrong, but realized that I could tell which was the principle, whether it turned out right or wrong."

Table II shows how these reasons are distributed.

TABLE II

PERCENTAGE DISTRIBUTION OF REASONS ACCORDING TO CLASSES

Reasons based on	Distrib'n acc. to age: All problems				Distrib'n according to problems											
	Age-Groups				Problem I				Problem II				Problem III			
	3-yr	4-yr	6–10-yr	Ad'ts	3-yr	4-yr	6–10-yr	Ad'ts	3-yr	4-yr	6–10-yr	Ad'ts	3-yr	4-yr	6–10-yr	Ad'ts
1. Observ'ns After Event	31	21			40	29			22	10			40	30		
2. Non-Specif. React'ns.	47	21	13		60	24	13		34	14	4		60	30	13	
3. Preferences	11	19	10				12		22	43	4				9	
4. Partic'ar F'tures	11	14	32	34			36	23	22	33	53	73			19	28
5. Spat. Pos'n		23	17	37		41	14	48			18	9		40	22	39
6. Former Succ. or Fail.		2	15	5		6	13	8			7				22	5
7. Changes in Sit'n			5	9			5	13				9			6	8
8. Social Factors			5	3			3				10	9			8	3
9. Suggested Signif'ce			2	7			3	2			4					12
10. Revision			1	3			1	4							1	3
11. Test Case				2				2								2
Total no. Reasons	19	48	376	134	5	17	231	48	9	21	28	22	5	10	117	64

1. The essential fact brought out by the first section of this table is that the reasons can be arranged in a series in such a way that there are reasons at one extreme used only by the young children, reasons at the other extreme used only by adults, near the middle a class used by all groups, and between this class and the extremes reasons used by two or more

adjacent groups. That is to say, on the basis of the distribution, which is objectively determined, the responses arranged themselves in a maturity series which represents a gradual transition from less mature to more mature modes of response.

In general, the older the group, the higher the point on the scale at which its reasons began and the higher the point to which its reasons extended, if 'higher' and 'lower' are used to refer to the more mature and less mature ends of the scale respectively. Furthermore, the reasons near the lower end of the scale were used more *frequently* by the younger groups, those near the upper end by the older groups. There are a few exceptions to this rule but the general tendency is indicated. Some of the percentages which make up this distribution are not well established, especially in the lower age-groups where the number of reasons reported was very small; but it is interesting to note that the general tendency is maintained with a fair degree of consistency.

2. The groups differed, too, in the range covered by their reasons. The children from six to ten had the widest range; but since this group is probably the least homogeneous in mental age, the wider distribution of their reasons is probably to be attributed to that fact. It should be noted, in this connection too, that the distributions for the different ages overlap and that the overlapping is such that the reasons given by each group begin at a higher point on the maturity scale and extend to a higher point than those given by the next younger group.

3. The right side of Table II indicates that the particular situation influenced the kind of reasons given. For instance, reasons based on particular features of the situation were present in all groups more frequently in Problem II than in either of the other problems, whereas space relations were used very rarely in Problem II, and rather frequently in Problems I and III. Of course, these two classes of reasons, which refer definitely to objective features of the situation would be expected to vary with changes in the problems presented. Even here, however, age-differences asserted themselves.

For example, the four-year-old group, which responded by giving concrete features of the situation as reasons in Problem II failed completely to do so in the other two problems; though the same problems elicited the response in question in the two older groups. The influence of both age and objective situation is indicated by this fact.

4. A tendency for the kind of reasons given to vary with a more general feature of the problem, namely its difficulty, is also indicated. There is some evidence that the less mature reasons are more frequent in the problems that are difficult for the group in question.

For instance, non-specific reasons and preference reasons, immature responses which appeared to a certain extent in the six-to-ten-year-old group, were more frequent in Problems I and III, which they found somewhat difficult, than in Problem II, which was easy for them. Possibly as the individual is confronted with a situation which he cannot easily manage, he is thrown back on a more primitive level of response. But this is not always the case. He may be stimulated to a more mature response by difficulty. With one exception, all reasons involving suggested significance, revision, and test cases, occurred in the more difficult problems.

In addition to these differences indicated by the distribution, there are sometimes, within the limits of a single class, qualitative differences in the responses of individuals of different ages.

For example, preference reasons used by children under five were usually stated in terms of simple personal likes and dislikes, whereas those used by older children were commonly stated in terms of somewhat more objective æsthetic standards. Besides, all preference reasons given by young children were for the boxes decorated with flowers; while in the older group they were not limited to this one situation, but were brought out by less obvious features of the drawings. Reasons based on particular features of the objective situation also revealed characteristic differences in the different age-groups. The flower in Problem II was the only feature that aroused this response in three- and four-year-old children; they ignored the triangles, circles, and other plain figures, probably because their experience had given them few or no associations with these rather abstract figures. The older children usually referred to them as stars, watches, tents, caps and the sun, suggesting concrete objects. Adults for the most part used the conventional names, circle, diamond, triangle, etc., and did not read concrete objects into them as the children did. Reasons based on space relations, too, showed characteristic age-differences. Though all the subjects tended to supplement their statements with gestures, this tendency was particularly noticeable in the two younger groups; and though responses on the basis of "nearer" and "farther" were more easily aroused in all groups than responses on the basis of "right" and "left," this reaction was much more apparent in younger than in older subjects. Social reasons, too, were different in children and adults. To children from six to ten, the fact that the experiment was being conducted by another person meant that the rules of the game were not inflexible. After making two or three correct reactions, a child would sometimes change his response and explain, "I thought you'd try to fool me and have the other one right." The adults, though aware that the conditions were being arranged by the experimenter, apparently assumed that a strict regularity would be observed. Their reasons took the form of speculations as to what rules the experimenter would be more likely to think of and use. Finally, reasons based on suggested significance differed in children and adults. As the illus-

trations show, the reasons of this class given by children were more elaborate, symbolic, and fanciful than those of adults, and were sometimes characterized by a touch of ethical significance.

Thus an examination of the responses within a single class confirms the general rule that there are characteristic differences in the reasons employed by different age-groups.

III. *Evidences of Development.*—A third general fact emerges from those previously stated and has in a sense been implied in presenting these. Both the quantitative and qualitative results indicate that the response of giving reasons is one which develops through the years; that there is an increase in the readiness with which it is elicited; that there is a progressive change in the forms it assumes; that there is an elimination and selection among the responses produced; in other words, that there is a growth or development which resembles the general trial-and-error procedure which psychological studies have so often found in the development of other complex organic processes, native and acquired.

It is probably worth while noting more specifically the conformity of the responses with the pattern of trial-and-error behavior. Varied reaction, which all trial-and-error presupposes, is indicated by the occurrence at every age and in every situation, of several different kinds of reasons; evidently the various tendencies represented,—tendencies to react to particular features, to spatial position, to suggested significance, etc.—are so related to each other that, though they possess different degrees of readiness, no one of them is invariably dominant over the others. Varied reaction of this sort would supply the necessary material on which the processes of stamping in and stamping out, whatever their nature, might operate. That processes resulting in elimination and selection are at work in some way is indicated by the nature of the distribution of the various classes of reason through the age-groups.

Table II shows that, as age increases, reasons which would generally be classified as inadequate—*e.g.*, reasons which disregard the accepted temporal order of cause and effect, which are merely matters of subjective preference, or which are non-specific—tend gradually to disappear; whereas reactions generally regarded as more adaptive—those which take into account objective features and relations, past experiences of success and failure, and possible significance on the basis of more remotely related associations—tend to appear and to be retained. Besides, the revision of a hypothesis by a more complete generalization or verbal

condensation is probably analogous to the formation of higher units in motor learning; and reaction on the basis of a test case probably presupposes a highly complex and generalized response based on many experiences with problematic situations. Both in general outline and in particular features the trial-and-error pattern is indicated, and the consistency with which the general trend of the evidence is maintained suggests a fairly definite direction of development.

This statement is made with recognition of the fact that the frequencies of the responses in question cannot be taken in any absolute sense. Even from this small sample of problem-solving it is clear that the nature of the objective situation has much to do with the kind of reasons given. The best single illustration of this fact is found in Problem II, where the four-year-old children responded in an almost adult manner, as judged from the frequency and also from the quality of the reasons given.

This probably means that if different problems had been used, perhaps less abstract in nature, a wider range of responses might have been aroused in the children. Some of the more mature reasons might have occurred; some of the less mature reasons might have been relatively less frequent. And if the adults had been placed in situations which were more difficult for them to cope with, they might have resorted more often to some of the less mature reactions. It is well known from everyday observation that even the least mature response on the list—that of using as reasons events which occurred after the reaction to be accounted for—has not been entirely eradicated from adult behavior; and experimental studies of the effect of changed data on the thought processes,[9] show that the responses are altered when the situation is changed even slightly. Undoubtedly the 'maturity' of a response is determined both by the objective situation and by the reacting organism.

There is, nevertheless, a definite basis for considering some kinds of reason more characteristic of mature subjects, others of immature subjects, and for regarding the general reaction of giving reasons as more characteristic of adults and older children than of children under five. When each problem is taken separately, there is a regular tendency for reasons to be used more frequently by the older than by the younger groups, and for some kinds of response to occur more often in adults, others in children; in other words for the character and frequency of the response to vary with maturity. The point is worth emphasizing that this tendency maintains itself in spite of changes in the objective situation which

[9] E. L. Thorndike, The effect of changed data upon reasoning, this JOURNAL, 1922, 5, 33–38; L. W. Kline, An experimental study in reasoning and its transference, *J. Phil., Psychol. &c.*, 1914, 2, 633–638; L. W. Kline & P. K. Anderson, The rôle of habit in reasoning, *School Sci. & Math.*, 1926, 26, 158–167.

effectively bring out changes in the response in question; even Problem II, which is exceptional from the point of view of the usual four-year-old performance, is no exception to this general rule. Over and above the changes in response induced by changes in the situation, the difference between more and less mature reactions appears.

Summary

In summarizing the results of this study, the following statements may be made concerning the reasons used by children and adults under the conditions described.

1. There were marked differences in the frequency with which individuals of different ages reported reasons during the solution of problems. The principal line of division came between the ages of five and six. The response was much more readily elicited from adults and older children than from children under five.

2. Differences in complexity also appeared. Adults and older children sometimes gave more than one reason for a single overt reaction, or a statement including several possible considerations. This response did not occur in children under five.

3. The reasons, when classified, and distributed according to age-groups, formed a series beginning with reasons used only by children under five, ending with those used only by adults, and containing between the extremes classes of reasons representing a gradation from less mature to more mature responses in the sense that at the lower or less mature end of the scale the reasons tended to decrease and at the upper end to increase in relative frequency throughout the age-groups. This series may be regarded as constituting an objectively determined maturity scale for the reasons reported in the condition described.

4. Within a single class on this maturity scale there were, in some cases, qualitative differences between the reasons used by younger and older groups.

5. The reasons reported by the various age-groups differed in range and showed considerable overlapping of the distri-

butions for the ages represented. The character of the overlapping was such that, as a rule, the reasons given by each age-group began at a higher point and extended to a higher point on the maturity scale than those given by the next younger group.

6. The objective situation influenced both the quantity and the quality of the reasons reported. In the midst of these differences, however, age-differences asserted themselves, the more and less mature responses occurring with greater relative frequency in the older and younger groups respectively.

7. Consistent age-differences, both in the readiness and character of the response, suggest that there is a regular development from less mature to more mature levels of activity in the reactions involved in giving reasons. This fact, together with the general trial-and-error character of the activity, suggests that the organism develops a tendency to give reasons in the same manner as it develops other complex responses, native and acquired; *i.e.* by the production of many and varied reactions and by the operation upon them of processes resulting in the elimination of some responses and the selection and fixation of others.

THE RÔLE OF FORM IN LEARNING

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The monumental works of Ebbinghaus, G. E. Müller and their pupils on memory have stood for a long time as the final word, not only on impressional, perseverative and reproductive tendencies, but also on learning and memory regarded at large. The notion that the learning of such material as nonsense syllables is typical of all learning and reproduction is based, however, upon the assumption of atomistic associationism, an assumption now severely questioned. Even a series of nonsense syllables, according to Ogden,[1] possesses some significant unitariness; although, like the conditioned reflex, it is a limiting case of a structured whole. "Memorizing is an activity involving a dynamic configuration of behavior. . . . Indeed, it is only when the series becomes a unified whole, with a certain pictorial composition, with a specific articulatory progression, or with beat, measure and melody, that the series can be memorized at all. . . . What is remembered is always a configuration, the members of which hold together precisely for the reason that they are the members of a configuration." [2]

In this study we shall not try to prove that memorizing would be impossible without some degree of unitariness. Other work may be cited in evidence of this fact.[3] We merely wish to demonstrate the effectiveness of a high degree of formal character in a series which is to be memorized. We have tried to put into a series something analogous to melody. Each series was constructed according to a general

[1] R. M. Ogden, *Psychology and education*, 1926, 187 ff.

[2] *Ibid.*, 210.

[3] K. Lewin, Die psychische Tätigkeit bei der Hemmung von Willensvorgängen und das Grundgesetz der Assoziation, *Zsch. f. Psychol.*, 1917, 77, 245 f; M. Smith & W. McDougall, Some experiments on learning and retention, *Brit. J. Psychol.*, 1920, 10, 204–207.

principle or scheme. When taken as a whole, it had a meaning or significance which was, if possible, new and unfamiliar.

Material. The following lists were devised for the experiments. Six series of numbers (*A, C, E, G, J, L*) were arranged according to some numerical progression. These will be designated hereafter as the '*Y*' series. Six other series (*B, D, F, H, K, M*), to be called '*X*' series, were made to correspond to them in length and type without having any particular formal character. And, finally, six series (*N, O, P, Q, R, S*), which we shall call the '*Z*' series, were constructed according to the plans of the first six but with different items. (See Lists 1 and 2.)

The chief problem with the number series was to determine quantitatively the degree of facilitation of learning where form was present and experienced, and to determine how such a form emerges in learning.

Another list was made of other materials, *i.e.* nonsense syllables with an orderly sequence of letters, series of French or English words, and series of letters, in all of which were concealed English sentences. (See List 3.) This material is so heterogeneous, however, that quantitative results are not significant. The forms were generally too obscure and did not emerge readily, at least during the few repetitions necessary for the learning of a series.

Subjects. Six naïve subjects took part in the experiments. They were enrolled in a second course in psychology, but they had had no laboratory experience in the subject.

Procedure. The members of the series were exposed one at a time in a rectangular window, 2 seconds for each item. After each exposure of the series S attempted to repeat the items. He was urged also to comment fully upon his learning. In this way some notion of the manner in which the forms emerged and of his attitude was usually obtained. A first group of 12 series, including *X* and *Y* series (List 1), was learned in the order, *L K J M H G E F C A D B*, thus alternating the *X* and *Y* series, and giving the *Y* series in the order of decreasing difficulty of form as nearly as could be determined before trial.

The instructions presented to S in printed form were as follows:

> You will be given a series of numbers, one at a time, which I want you to learn. Say each one to yourself as it appears in the window. After each trial I will ask you to repeat as many members as you can, in the correct order if possible. You will also be asked to make any remarks which you can think of in connection with your learning of the series.

The remaining six of the eighteen *X*, *Y* and *Z* series (List 2), which possessed the same forms as those in the first group, were given in the order, *Q R S P O N*, and with the following revised instructions:

> You are to learn the series as before. Each series is constructed according to some principle or scheme, or, taken as a whole, it has some meaning. Watch for the scheme, but at the same time go on memorizing the series in the usual way until it occurs to you.

The series of nonsense syllables, words and letters were mixed in with the number series, half of them under the earlier instructions and the other half under the later. Under the early instructions S had to discover for himself that there was form in the

List 1

X and Y Series

A	*B*	*C*	*D*	*E*	*F*	*G*	*H*	*J*	*K*	*L*	*M*
5	2	94	96	2	2	8	7	3	7	13	12
6	3	81	87	5	6	7	12	5	9	16	14
8	7	69	64	3	9	9	1	8	4	11	10
11	10	58	55	6	7	6	5	12	8	14	16
15	19	48	41	4	4	10	11	4	5	9	7
20	24	39	31	7	8	5	14	6	14	12	13
26	29	31	30	5	1	11	2	9	6	7	9
33	32	24	24	8	3	4	8	13	10	10	11
41	48	18	17	6	5	12	6	5	13	5	3
50	58	13	12	9	8	3	10	7	6	8	6
60	65	9	9	7	6	15	3	10	3	3	5
71	72	6	5	10	4	2	9	14	8	6	8
83	88	4	3	8	10	14	13	6	11	1	4
96	91	3	1	11	5	1	4	8	15	4	1
								11	12		
								15	5		

List 2

Z Series

N	*O*	*P*	*Q*	*R*	*S*
8	97	5	9	5	15
9	84	8	8	7	18
11	72	6	10	10	13
14	61	9	7	14	16
18	51	7	11	6	11
23	42	10	6	8	14
29	34	8	12	11	9
36	27	11	5	15	12
44	21	9	13	7	7
53	16	12	4	9	10
63	12	10	14	12	5
74	9	13	3	16	8
86	7	11	15	8	3
99	6	14	2	10	6
				13	
				17	

series. There were individual differences in this respect; one S found the intended form in the first *Y* series which he learned, whereas another S found none until the later instructions were given. Once form is found, S is under some degree of self-instruction to look for it in every series.

List 3

1	2	3	4	5	6	7	8	9	10
ZAN	FAZ	C	G	que	ou	con	myc	force	thick
YED	DEY	D	Q	ne	des	sta	oun	Korans	wall
XIF	GIX	B	T	beau	ni	nti	try	heaven	it
WOJ	QOW	M	U	dit	beau	nop	rig	ye	tea
VUL	NUV	N	R	qui	dit	lew	hto	sago	of
UAN	MAU	O	N	sabot	qui	ast	rwr	are	myrrh
TEP	ZET	B	G	dis	la	hec	ong	feathers	seize
SIC	BIS	S	4	ni	des	api	sti	brat	knot
ROS	VOR	A	O	de	ci	tal	llm	four	strain
QUV	LUQ	R	I	beau	or	oft	yco	than	debt
PAK	KAP	2	C	dit	pense	het	unt	this	drop
OEZ	PEO	O	U	elle	si	urk	rys	count	path
NIF	XIN	S	R	tra	d'ou	sof	hes	anent	Asa
MOJ	JOM	I	M	la	tel	old	aid	anew	gent
LUP	WUL	C	T					neigh	tell
								shun	reign

Results.—The results by series for all Ss is given in Table I. A number italicized means that the form emerged before the final exposure required for one correct recitation. A number set in blackface means that the same form had emerged before.

Table I

Subj.	*A*	*B*	*C*	*D*	*E*	*F*	*G*	*H*	*J*	*K*	*L*	*M*	*N*	*O*	*P*	*Q*	*R*	*S*
B.	3	2	3	2	4	3	3	3	4	8	5	3	*1*	*1*	*1*	3	*2*	3
H.	*1*	3	2	3	*1*	3	*2*	5	*1*	9	5	2	1	*1*	1	1	2	2
L.	2	3	4	2	*1*	4	4	3	13	6	7	3	4	7	1	1	*3*	*2*
Mc	*1*	5	10	4	*1*	4	*1*	5	*3*	13	*3*	3	*1*	*2*	1	1	1	1
M.	*1*	3	3	3	*1*	4	*2*	3	6	8	10	3	1	*2*	1	2	*1*	*1*
S.	*2*	3	3	3	*1*	3	*2*	3	*3*	4	5	4	1	*2*	1	2	1	*1*

We may ask several questions about these results. (1) Does the emergence of form facilitate learning to any great degree? (2) Does failure to emerge when it is expected interfere with learning? (3) Is there a transfer of learning through the learning of certain forms?

The last two questions arise from considerations falling outside our problem and from casual observations made during the experiments. Expectancy, as pointed out by Ogden,[4] is one of the important conditions for learning, and

[4] *Op. cit.*, 248.

one which has been often overlooked. Expectancy sets an end which requires fulfillment, and if this end is not fulfilled in some way the result is dismay. The comments of our Ss satisfy this general description in all cases where an expected form did not emerge. Will quantitative results tell the same story? According to the same author, an experienced configuration is the secret of transfer of training.[5] The form, like a melody, is transposable, and may be clothed in new members. Will the forms residing in our number series be transposable in the same way?

We can answer the first question by comparing the results from the *X* series with those from the *Y*. Those from the *Y* series must be subdivided into the cases in which the form emerged and those in which it did not. These cases will be known as *Y*1 and *Y*2 respectively. If form is an aid, the average number of trials in learning an *X* series should be greater than that in learning a *Y*1 series. Again, the number of trials in learning a *Y*2 should be greater than in learning a *Y*1 series. This is true for every S and also for the combined averages.

TABLE II

Subj.	*X*	*Y*1	*Y*2	*Z*1	*Z*2	*Z*3
B	2.6	—	3.6	—	1.0	3.0
H	3.2	1.3	3.5	1.2	1.0	—
L	3.0	1.0	4.2	1.0	2.0	5.5
Mc	4.2	1.5	10.0	1.0	1.5	—
M	3.2	1.3	6.5	1.3	1.5	—
S	3.2	1.7	4.0	1.3	1.5	—
All (av.)	3.2	1.4	4.6	1.2	1.4	4.25
Cases	30	16	14	15	11	4
P.E.$_{av.}$	.11	.12	.39	.07	.07	.55

The results from the *Z* series, in Table II, are subdivided as follows; *Z*1, where the form emerged for the second time; *Z*2, where the form emerged only once; and *Z*3, where the form did not emerge at all.

When an expected form fails to emerge, the interference with learning is indicated in two ways. The averages in the *Z*3 series were greater than those in the *X* series, and even

[5] *Ibid.*, 258 ff, 279 f.

those in $Y2$ were greater than those in X; although the $Y2$ group contained several cases where form was not yet expected or known to be present. This result agrees with numerous comments of the Ss to the effect that when looking for form they failed to learn the items of the series.

Transfer through form is strongly evident in several ways. The results for $Y1$ are usually greater than those for $Z1$; a form is more quickly learned the second time. Results for $Z2$ are also usually greater than those for $Z1$. The importance of the form over the members of the series was shown in the many cases of recitation during which the S was doubtful about certain items, or else did not know them at all except through the demands of the scheme.

On one occasion an S declared that he had learned a certain Z series before, whereas the members were entirely new. Often a correct recital was prevented merely by the fact that S failed to notice the beginning and final members of the series. Many of the forms were continuous, *i.e.* had no 'natural' beginning or ending, were not 'closed' forms; they were lacking in precision at the ends except for that precision which belongs to any series and which usually permits an S to retain the first and last members easier than the middle members.

We may ask whether implicit form aids learning; whether the Y series are more readily learned than the X even if S does not experience the intended form. If this were true, we might expect the results in the $Y2$ series to be less than those in the X series, but such is not the case. We have already pointed out that the delayed learning in the $Y2$ series was due to interference occasioned by the failure to find a form where one was expected.

If we take only those $Y2$ series, nine cases in all, which were learned before the Ss had discovered any form at all, and compare them with the corresponding X series, the difference is still in favor of the X series; 3.0 trials for the X series, and 3.6 trials for the $Y2$ series. If a conclusion is warranted from so small a number of cases it must be that implicit form has no influence on learning. In order to be effective in learning, a form must be somehow represented as such in the experience of the individual.

If we combine the results for the six Ss, thus neglecting individual differences in order to get more cases, statistical tests for significant differences can be made (Table III).

TABLE III

Series	Diff.	P.E.$_D$	D/P.E.$_D$	P.C.
X–Y_1	1.8	.17	10.59	1.000
X–Z_1	2.0	.13	15.39	1.000
X–Z_2	1.8	.13	13.92	1.000
Y_2–X	1.4	.40	3.50	.982
Z_3–X	1.45	.57	2.55	.914
Z_2–Z_1	.2	.10	2.00	.823
Y_1–Z_1	.2	.14	1.43	.665

Here the first three differences are well above statistical certainty, confirming the previous conclusion that the emergence of form always facilitates learning. The next two differences are almost certain to be universal, demonstrating again that the failure of form to emerge, when it is expected, delays learning. The evidence for transfer through form is not nearly so certain; although there is a degree of probability in its favor.

One thing which undoubtedly worked against the effect of transfer was that a different form can emerge in the same series upon a second appearance. Series *G*, for example, may be conceived either in pairs of numbers diverging each time by one unit, or it can appear as two alternating series, one ascending, the other descending, or as a series with increasing differences between successive members, alternating in sign. Transfer by way of form should take place only when the same form arises on a later occasion. Our own cases in which a new form arose on the second occasion were so rare, however, that no quantitative comparisons are warranted.

What can we say qualitatively concerning the way in which form emerges under the conditions of these experiments? It must be mentioned again that the Ss were naïve and unused to introspective methods. Certain deductions can be made from their comments, however, which are quite full. We may ask first of all whether the forms emerge suddenly, all at once, or gradually and continuously, or step by step. Do they come piecemeal, by way of an association of elements? or do they come by way of a general outline, the detailed structure of which is later filled in?

They do not appear to come all at once completely structured in their final form unless the pattern is extremely simple or familiar. Neither do they appear to come by way of a continuous growth; but they do come step by step. The kind of step varies with the kind of materials, and with

the difficulty of the form. On the whole, the forms in the number series emerge first of all as a generalized pattern. We can perhaps distinguish the following steps, which are more apparent in the more difficult series.

1. A feeling of regularity or order, as attested by the following comments:

"There were glimmerings of a scheme all the way through this but I couldn't get it precisely" (Series *G*, after the first trial). "There was such regularity in that one that I did not notice the numbers well enough" (Series *G*, after the first trial). And yet, in these instances, the Ss were unable to describe any scheme.

It is questionable, however, whether this feeling of regularity is a universal or necessary prelude to the process. A feeling of regularity or order might occur in the learning of an irregular or orderless series. This question should be tested with a large number of series with and without formal character to see whether the feeling is 'illusory' or dependent upon a realizable order.

2. Awareness of direction.

"I started looking for a scheme and saw none except that they were arranged in order of descending quantity" (Series *C*, first trial). "The series is going down all the time" (Series *D*, first trial).

3. Specific nature of direction.

"I noticed a natural sequence, a number in each ten's place from 30 to 90" (Series *A*, third trial). "I noticed that the numbers are coming in a regular progression; they go up; there are four of them" (Series *J*, second trial). "Noticed a scheme of four numbers, beginning with a digit and increasing and then dropping again and so on" (Series *R*, first trial).

4. Specific relationship of members.

"I noticed more relationship this time. The second number (in each group of four, series *R*) is two more than the first; the third is three more than the second; the fourth is four more than the third" (Second trial). "There is a difference of three in each pair, starting with 2 and going up to 8" (Series *E*, first trial).

Not all series require all these discrete steps, for the whole process often occurs during one repetition. On the other hand, some of the steps may be carried through more than one repetition. We have merely tried to picture the process of emergence as it can be pieced together from scattered comments. The chief thing to be noted is the direction of the process, more general forms coming first, with a more detailed structure coming later. If this analysis be accepted,

the form does not arise from the piecing together by association of discrete elements and relations. The elements become merely the necessary members of the complete form and are remembered as such.

The rôle of relations in the learning process is not clear. By relations we mean the relationships of each member to its near neighbors in the series. There is a tendency, especially after S has discovered that forms of a certain kind are present in some of the series, for him to build up the scheme by induction. He notes the relation of each member to the next, watches for repetitions of these relationships and gets the whole scheme from these 'samples.' It was a frequent occurrence for S to report that he got the scheme from the first three or four numbers in the series without having to see the whole series. We may say, however, that the relationships themselves are forms, over and above their elements, and that in so far as S projects them into a tentative scheme for the whole series they are, by virtue of imagination, the unifying form of the whole series. This tentative form would be discarded for another as new-appearing items fail to fit the scheme. The relations would therefore be secondary to the total scheme once the latter has emerged. This is probably the reason why S finds it difficult to verbalise the scheme; although it is the relations which he has to put into words in order to describe the form.

In the other series, the process seems to be somewhat different. The series of nonsense syllables and of words possess forms which did not readily emerge; consequently, data from these series are very meager. As for the series of nonsense syllables we can say that the two principles, the order of initial, or final, consonants and that of the middle vowels, came independently and all at once. In the series of words which contain sentences, the form was built up step by step. The following case is typical (Series 14). "I begin to see something now. It must be 'the quality'" (after the first trial). "I get 'as a gentle rain' at the end" (after the fifth trial). "I get it all now" (after the sixth trial).

In conclusion, we may say that a highly unitary form in a series of numbers, even though unfamiliar, greatly facilitates learning, and that the failure of form to emerge, when one of a certain kind is expected, interferes with memorizing. Form takes precedence over the members of the series and their interrelations, and once experienced it emerges more readily in a series possessing new members. Under the conditions of our experiment, the forms in series of numbers emerged step by step, and in direction from a general, outlined form to a more particular, structured form; although form may also come by way of successive inductions based upon the interrelations of the members. It is our notion that the material and the methods of learning used here approximate more nearly the normal case of human learning than do series of nonsense materials learned by sheer force of repetition.

THE RÔLE OF PRACTICE IN SPEED OF ASSOCIATION

BY FREDERICK H. LUND

Barnard College, Columbia University

Proficiency in a given performance is usually measured in terms of the speed and effectiveness with which the task is executed. The results of experiments on learning and practice would seem to indicate that such proficiency is influenced by four factors, each of which may vary independently: (1) the ease with which the individual motor adjustments are carried into effect, (2) the ease with which the elements calling for adjustment are discriminated, (3) the ease with which the recognized sensory elements lead over into appropriate reactions, that is, the facility of the associative and coördinating processes, and (4) the ease with which the shift between consecutive adjustments is made. In other words, the typical adjustmental situation calls for *motor adjustment*, *sensory discrimination*, *association* and *shift*.

The factors mentioned pertain only to the perfection of *necessary* adjustments, while differences in proficiency have long been recognized to depend upon still another factor, the presence or absence of *unnecessary* reactions. In skilled performances there are always a certain number of 'random reactions' which tend to be eliminated with increased practise. This is true even in the simplest tasks where the reactions to be made are closely defined and well known to the subject. But while the importance of this factor is fairly obvious and has been duly recognized in practise and learning experiments, the same cannot be said of the other four factors, the significance of which is much more difficult to determine experimentally.

The present study, while conducted with respect to certain tests, is believed to have a more general implication regarding the manner and extent to which these four factors influence efficiency and are modified through practice.

The problem arose in connection with the color-naming test. This simple test affords a good opportunity for analysis, since the factors under consideration are all present and easily differentiated. Woodworth and Wells[1] included it among their

[1] R. S. Woodworth & F. L. Wells, Association tests, *Psychol. Monog.*, 1911, 13 (no. 57).

association tests. It had originally been devised for the purpose of measuring the speed of the individual's associative processes and his general capacity for naming familiar objects.[2, 3] That is to say, it was believed that the subject's 'associative performance' in this particular case would furnish an indication of his general 'associative capacity.' It must be realized, however, that the subject's performance in this instance would depend upon a number of other conditions besides an assumed native 'associative capacity.' Foremost among these conditions would be his familiarity with this particular function, or his familiarity with the *specific* associations called for in naming the colors.

In entering upon a systematic analysis of color-naming and one or two other related tests, we shall consider in turn the relative importance of the four factors which we have enumerated.

The Motor Adjustments. In determining the conditions influencing proficiency in color-naming the motor aspect of the performance was the first to attract the interest of investigators. Woodworth and Wells, observing the frequent hesitation and obstruction experienced by the subject in naming the colors, referred these 'inhibitions and interferences' to the "mutual interference of the five names, all of which, from immediately preceding use, are on the 'tip of the tongue,' all equally ready and, therefore, likely to get into one another's way." [4] This interpretation, suggested tentatively by these authors, found general acceptance among later investigators. That the 'obstructive tendencies' are attributable to difficulties in articulation, *i.e.*, to the equal readiness of the color responses as determined by immediately preceding use, is disputed, first by Hollingworth and later by V. W. Lemmon,[5, 6] who point out that the same series of speech reactions are required when the subjects read a comparable list of printed color-words in which case the obstructions are *absent.*

This conclusion is confirmed by an experiment in which the writer used 28 subjects [7] in color-naming and what might be

[2] J. M. Cattell & L. Farrand, Physical and mental measurements of students of Columbia University, *Psychol. Rev.*, 1896, 3, 618–647.

[3] C. Wissler, The correlation of mental and physical tests, *Psychol. Rev. Monog. Suppl.*, 1901 (no. 16).

[4] H. L. Hollingworth, Articulation and association, *J. Educ. Psychol.*, 1915, 6, 99–105.

[5] V. W. Lemmon, Master's thesis (unpubl.), Columbia University, 1923.

[6] H. E. Garrett & V. W. Lemmon, An analysis of several well-known tests, *J. Appl. Psychol.*, 1924, 8, 424–439.

[7] Freshmen at Barnard College.

called "word-naming." In color-naming the standard sheet prepared by Woodworth and Wells was used. In word-naming another sheet was prepared with the five color-names typewritten, each twenty times, following the sequence and spacing of the color squares on the standard sheet. The results (see below) show that it took, on the average, 56.0 sec to name the colors and only 36.4 sec to read the words, giving a ratio of about 3 : 2 in favor of word-naming. The difficulty in color-naming, referred to by Woodworth and Wells, cannot, then, be sought in the motor aspect of the performance, since this is the same for color-naming and word-naming, and since in the one the obstruction occurs and in the other it is absent.

	Ave.	M. V.
Color-naming	56.0	6.3
Form-naming	80.4	9.0
Word-naming	36.4	2.9
Color-finding	55.6	7.3
Form-finding	72.9	9.6
Word-finding	100.3	9.4
Color-naming 'without discrimination'	33.3	3.7
Form-naming 'without discrimination'	41.2	4.8
Word-naming 'without discrimination'	33.0	3.2

Sensory Discrimination. The ease with which the sensory elements can be distinguished becomes a factor, as we have observed, in determining the speed of performance. The more alike the stimuli the greater will be the opportunity for confusion. It is possible, therefore, that the greater difficulty of color-naming as compared with word-naming is due to the greater discriminability of the sensory elements in the latter. The reverse, however, proved to be true when the matter was put to test. Instead of naming the colors consecutively the subjects were required first to name all the reds (following the lines down the page with a pencil), then all the yellows, and, in turn, all the greens, blues and blacks. This was called the 'color-finding' test. The same procedure was used in 'form-finding' [8] and 'word-finding.' From the above table it will

[8] In form-naming and in form-finding the standard substitution test blank was used. The form-naming test is well adapted for purposes of comparison in this case since it contains the same number of items as the color-naming test.

be seen that 'word-finding' took almost twice as long as 'color-finding.' The subjects were agreed that the particular color-squares attended to would 'stand out' so much more clearly and that this accounted for the difference in speed of performance. This deduction was supported by a further test in which discrimination was reduced to a minimum, the different colors being presented on separate sheets but spaced as in the original. The same adaptation was made for the words and the geometrical forms. In the table the results of this test are labeled 'without discrimination.' They show that the time for color-naming and word-naming under these conditions is the same. That form-naming 'without discrimination' should require more time is to be expected since it contains two more syllables which must be repeated twenty times.

Ease of Shift. A performance which contains any degree of complexity must require successive shifts as the subject passes from one adjustment to the next. But these adjustments veiwed as motor phenomena cannot account for the difficulty under consideration since the successive motor shifts are the same in color-naming and word-naming.

Speed of Association. Since no satisfactory account of the particular difficulties experienced in the color-naming test has been found either in the assumed obstructive tendencies in the motor performance, in the discriminability of the sensory elements, or in the shift phenomenon, we are forced to look to the associative process as the important differential.

The speed and facility of the associational process may vary with two factors; the *complexity* of the process and the *practice* obtaining in the function. Subjects when questioned as to their apparent difficulties in color-naming as compared with word-naming were inclined to feel that there was a difference in the *complexity* of the two functions; that there was 'something more' in color-naming than in 'merely reading the color names.' The latter, they contended, is 'much simpler because you have the word right there and do not have first to think of this word as in color-naming.' Thus it seemed to the subjects that there was an extra step involved

and that the verbal response 'red,' for example, is more closely identified with the printed word 'red' than with the red color square. Yet the vocal innervation, or its auditory equivalent, is no more like the one of these visual stimuli than the other. Doubtless everyone has noticed how constant association of a vocal element with a certain sensory element gradually brings about an identification of the two, until they appear 'similar,' such similarity being present neither as a sensory nor motor feature but merely as an associational fact.

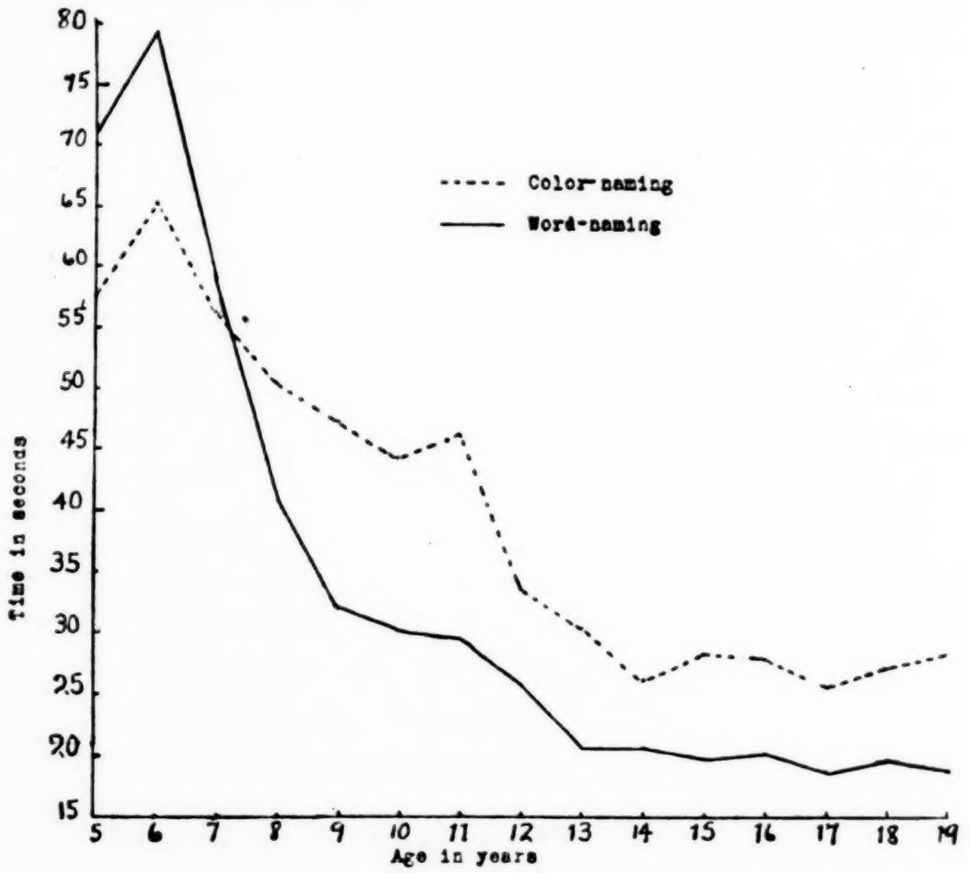

FIG. 1. Average performance in color-naming and word-naming for various age-levels

Since the motor response is the same whether we respond to the color square or to the printed symbol, and since there is no inherent circumstance which should make it harder to respond to the one visual element than to the other, the difference in the capacity to perform the two tests must be attributable to *practice*. Experimental determination of this point might be possible along two different lines: one might give adult

subjects further practice in the two tests and note whether this tended to equalize the time required in the two performances; or one might test subjects at an age when practice in the two functions had been equal.

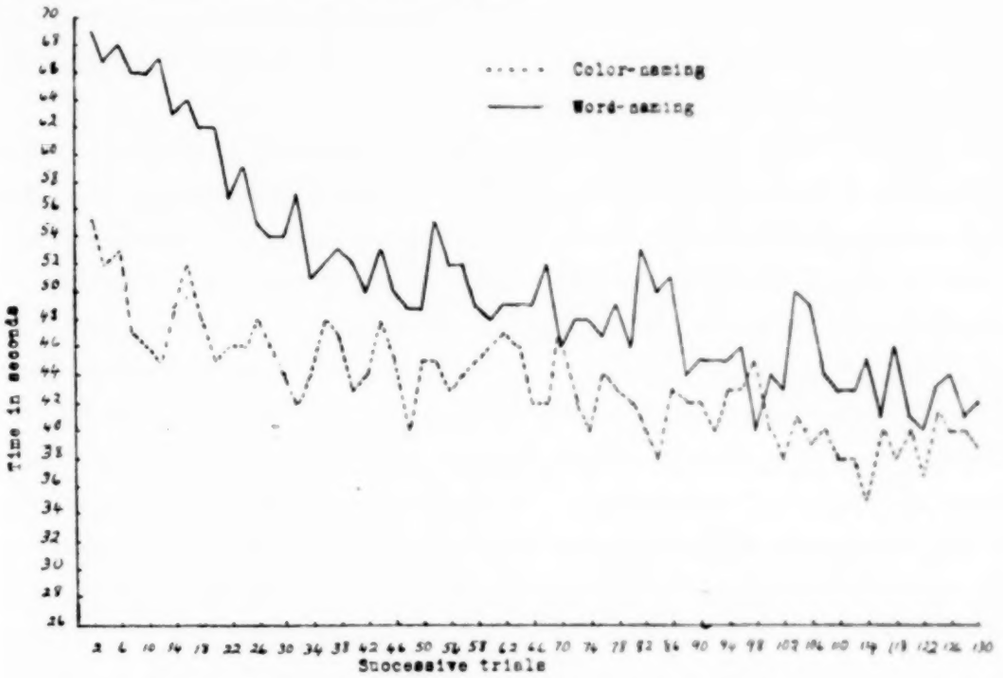

FIG. 2. Effects of practice upon speed of performance in color-naming and word-naming (E. B., a five-year-old subject)

As a method of approach the latter should doubtless be the more convincing if subjects could be found whose practice was demonstrably equal. This proved to be unnecessary, however, since it was found that children before the reading-age, having had more practice in responding verbally to colors, did *better* in color-naming than in word-naming even after being given a sufficient number of preliminary trials to make sure they knew the words and the required responses. Figure 1 gives the average performance of 65 subjects ranging in age from 5 to 19. The results cannot be regarded as norms for these age-levels since only five or six subjects on the average were tested at each level. With larger groups the curves might be expected to show greater regularity and more uniform increase in capacity with advancing age. In order to attain a desired regularity in the performance of the younger subjects it was necessary throughout to shorten the test using only 50 color-squares and 50 words.

Still more convincing as to the importance of practice was a more intensive experiment in which a five-year-old subject, E. B., was put through 130 trials in color-naming alternating with 130 trials in word-naming. The successive trials which were spread over a period of four weeks showed that performance in the two tests tended to approach the same level with increased practice (Fig. 2).

With these results it is scarcely possible to doubt that practice is the chief determinant of the difference in the ease of performance in the two tests under consideration. The association between the printed stimuli and the verbal responses has occurred with such frequency as to render the process more or less automatic. The associative connections, having been exercised much less frequently in the case of the colors, function with less ease and require more effort. The *type* of associative process represented in the two tests is the same, however, since it is formed on equally arbitrary grounds, there being no more necessary relation between the printed stimulus and the verbal response than between the color stimulus and the same response.

Brown[9] arrives at somewhat different conclusions from his study of the color-naming test. He put his subjects through a practice series of 12 days in color-naming and word-naming and concluded that 'the difference in speed between color-naming and word-reading does not depend on practice.' In vindication of this statement he points out that even after prolonged practice (working with adult subjects) "absolute rate in naming colors remains much slower. . . . Furthermore, the life-long practice which we have had in reading words has not brought that function to its maximum." But, in the first place, it is doubtful whether we ever reach a 'maximum' in any function, and, in the second place, it should be observed that 'our life-long practice' (if this expression is justifiable) has been in responding to printed symbols in various arrangements, and not in this *particular* arrangement. The formation of serial connections between the elements should be sufficient to account for the improvement which occurs.

Rather than negating the significance we have attributed to practice Brown's experiment may be seen to confirm it. No other interpretation can account for the fact that his subjects improve more in color-naming than in word-naming. This appears not only when the absolute difference between initial and final ability is considered, but also when the percentage of improvement is taken into account. While in word-naming the improvement is only 17 per cent., in color-naming it is 26 per cent. Brown himself observes that when the initial and final performance is

[9] Warner Brown, Practise in associating color-names with color, *Psychol. Rev.*, 1915, **22**, 45–55.

considered the ratio (time for colors divided by the time for words) between the two performances is reduced from 1.59 to 1.41, which would indicate that as we equalize practice the time required for the performances tends to be equalized.

The results presented above have important implications for certain other problems, some of which we shall briefly consider.

Practice Level and Physiological Limit. In many experimental investigations it has been necessary to bring the subject up to an assumed 'practice level' before starting the actual test. This has been true when the experimenter has been interested in the influence upon performance of other factors (temperature, food, drugs, etc.) besides practice. Similarly, in experimental studies of learning and practice frequent mention has been made of what is called a 'physiological limit,' or a point reached in proficiency which marks the upper limit of the individual's capacity. Our materials tend to cast doubt upon these concepts. The fact that after years of practice in responding verbally to printed symbols there is still evidence of improvement would indicate that the 'levels' or 'limits' in question cannot actually be determined, and that it would be truer to say that they exist only as theoretical limits which one *tends to approach* with increased practice.

Testing 'Intelligence.' Our tests of intelligence purport to measure not education but educability, not information but native adaptational capacity. This being the case, should our criterion of an individual's capacity be his *speed* of performance or the *excellence* of performance (that is, the point reached on a scale of difficulty), or should both speed and excellence be considered? The relative significance of these two variables has, I believe, never been determined. It is obvious, however, that most of the tests now in use give chief importance to speed. This is no doubt because one of the easiest scales to construct is the one which makes amount per unit of time the determining variable. Another fact to be observed is the emphasis placed on linguistic capacity which gives a decided advantage to individuals with facile verbal mechanisms. Having made speed of verbal reactions the

significant differential we can hardly be said to be measuring any native factor, but rather the practice an individual has had in the type of responses called for. Under these conditions it is not surprising that an individual's I.Q. (presumably a constant) should rise or fall depending upon the extensiveness and *form* of his education, or that his I.Q. should tend to increase with each testing. And, again, the observation frequently made that women do better than men on verbal tests might be attributable to a difference in the amount of practice in functions of this order and not to more facile associational mechanisms, as such, since their reaction time is not appreciably shorter.

The Effects of Practice upon Individual Differences. The significance of practice as compared with native ability is seen again in the disposition for individuals (in some tests at least) to become more alike through practice. This is true not only when the absolute difference between initial and final performance of the best and poorest subjects is considered, but also when the ratio between the scores of the best and poorest subjects' initial performance is compared with the ratio of the final scores of the best and poorest subjects. In color-naming after 100 repetitions, this ratio, according to Hollingworth,[10] was reduced from 1 : 3 to 3 : 7, and in naming opposites, after the same number of repetitions, from 1 : 11 to 2 : 5. This would indicate that with practice the variability in group performance is reduced and that the reduction is greatest in the more difficult performance.

It has also been found (Hollingworth, Gates [11] and others) that with practice the correlations between the standing of individuals in different performances tend to increase. This increase has usually been ascribed to the disposition of individuals to approach their 'ultimate capacity,' or a limit of performance determined by native constitution. That performance tends to become steadier with practice may be sufficient, however, to account for the increased correlation,

[10] *Op. cit.*, p. 101.

[11] G. S. Gates, Individual differences as affected by practice, *Arch. of Psychol.*, 1922 (no. 58).

and, in turn, the increase in steadiness may be largely due to the fact that with further practice the general practice-level attained in the past is asserting itself more and more. It is plausible at least that such a general practice-level is equally important with native ability in determining the increase in correlation.

Transfer of Training. If, in measuring an individual's height during successive years of growth, one should place him on a pedestal and include the pedestal in his measurement, one would obviously get a smaller percentage of growth for these periods. Now, in the transfer experiment, when we measure the percentage of improvement in the practice series or the percentage of transfer to the related series, we are doubtless including in our measurements, if not pedestals, then what approaches the same thing, since equal improvement may not be expected through practice in all aspects of the functions tested. For example, if one should use the color-naming test in the practice series, and test before and after in form-naming, the percentage of improvement and of transfer would decidedly increase if the time for articulation were excluded, since articulation as such could not be expected to show appreciable improvement. If practice in one function has any value for performance in another, it must be in the amount the subject is able to profit from practice in the elements common to both. Since these elements will not show equal practice-effects, an analysis of the transfer situation should include not only a determination of common elements, but a determination of the extent to which each of these may be expected to improve through practice; that is to say, the extent to which each of these elements constitutes a 'pedestal' depending upon their individual levels of practice.

THE PSYCHOLOGICAL LABORATORY OF OHIO STATE UNIVERSITY

BY ALBERT P. WEISS

At Ohio State University the Department of Psychology [1] is responsible for all the official courses in psychology given on the campus. From the beginning it has been the administrative policy to encourage without prejudice all recognized phases of psychology and to retain them under a unitary organization. The wisdom of such a policy is written in its growth. In 1912 the faculty of the department consisted of two professors and one laboratory assistant with an enrollment of approximately 200 students. In 1926 the permanent teaching staff consisted of twenty-two members assisted by thirteen laboratory and graduate assistants. The student enrollment, graduate and undergraduate, has increased to 5,000.

In order to preserve and, if possible, to increase the autonomy of the individual members, and to simplify the administrative problems, Professor George F. Arps, Head of the Department, has distributed the administrative and academic problems into five major divisions. Under this form of unification and administration all the departmental resources in specific training and laboratory equipment are at the disposal of the whole personnel.

Administrative Divisions

The general administration and coördination of the several divisions are under Professor George F. Arps.

[1] The writer was requested to prepare this report by reason of his connection with the department during the period of its development. Chief among those factors which contributed toward this development were the hearty support and prompt coöperation of the University Administration and the Graduate School. Many others besides those mentioned were consulted. Acknowledgment is made of the valuable services rendered by Dr. E. A. Doll, Director of Research, The Training School, Vineland, N. J., during his connection with the department.

1. General, experimental and comparative psychology are under Professor Albert P. Weiss. The instruction in elementary psychology is in charge of Professor William R. Wilson.

2. Abnormal and clinical psychology under Professor H. H. Goddard.

3. Educational psychology under Professor S. L. Pressey.

4. Industrial psychology under Professor H. E. Burtt.

5. Statistics and university intelligence tests under Professor H. A. Toops.

The above order roughly approximates the order of development of these fields at Ohio State University.

General Administration

The University is administered under the four-quarter (per year) plan. During the year about sixty different courses in psychology are offered. The enrollment for the academic year 1925–1926 (autumn, winter, spring, and summer quarters) was 3,189 in the elementary courses and

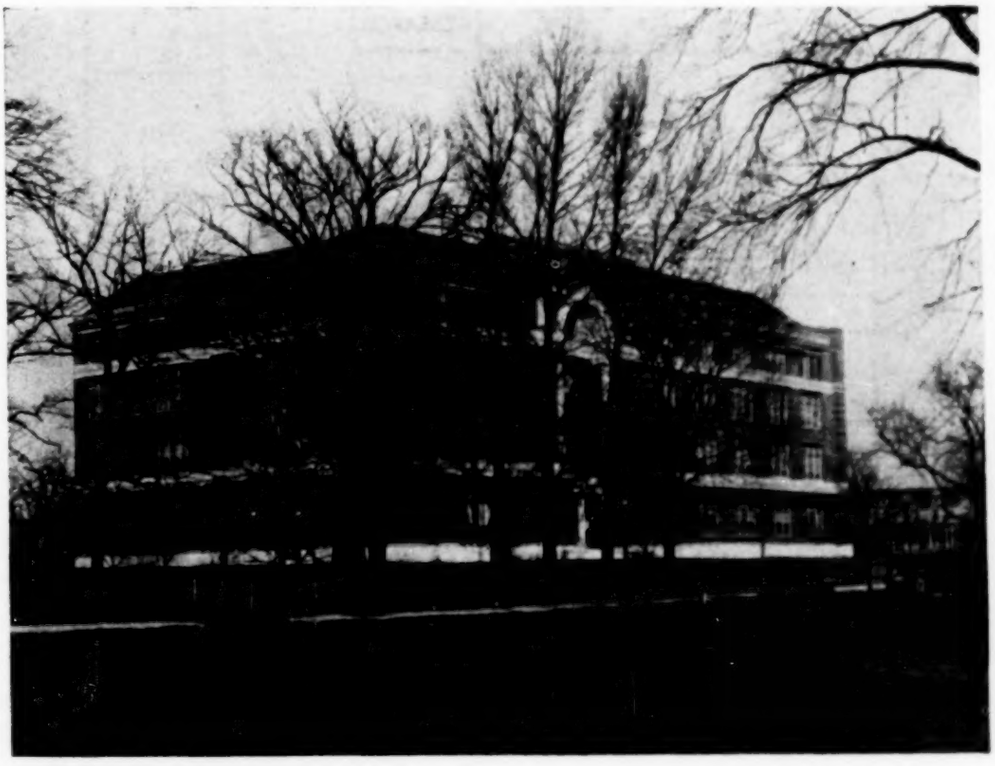

Fig. 1

1,932 in the advanced courses, a total of 5,121 enrollments (for the four quarters).

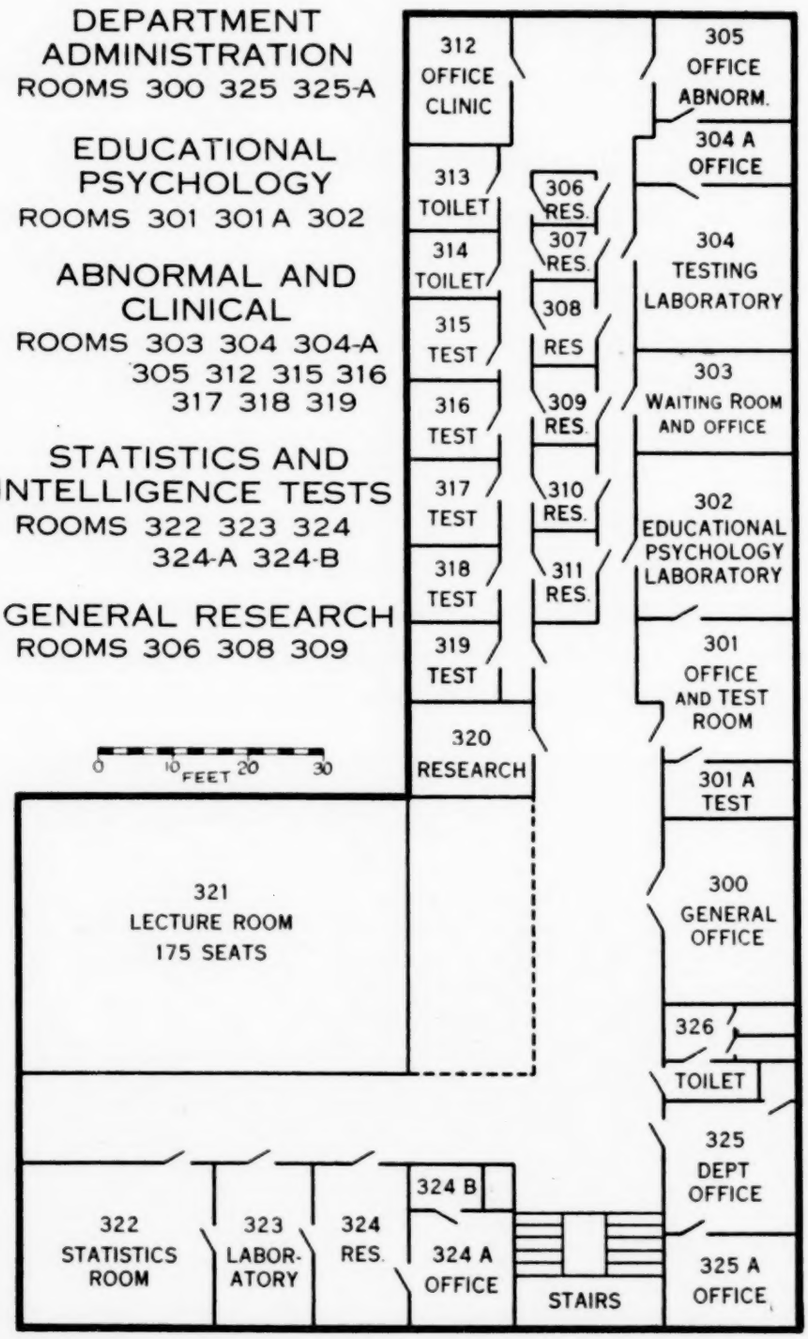

FIG. 2

The personnel consists of six professors of full rank, five assistant professors, eleven instructors, thirteen laboratory and graduate assistants, fellows and scholars, and three full-time stenographers.

The department is located on the third and fourth floors of the new Education Building (Fig. 1), the first unit of which was completed in the Spring of 1926.

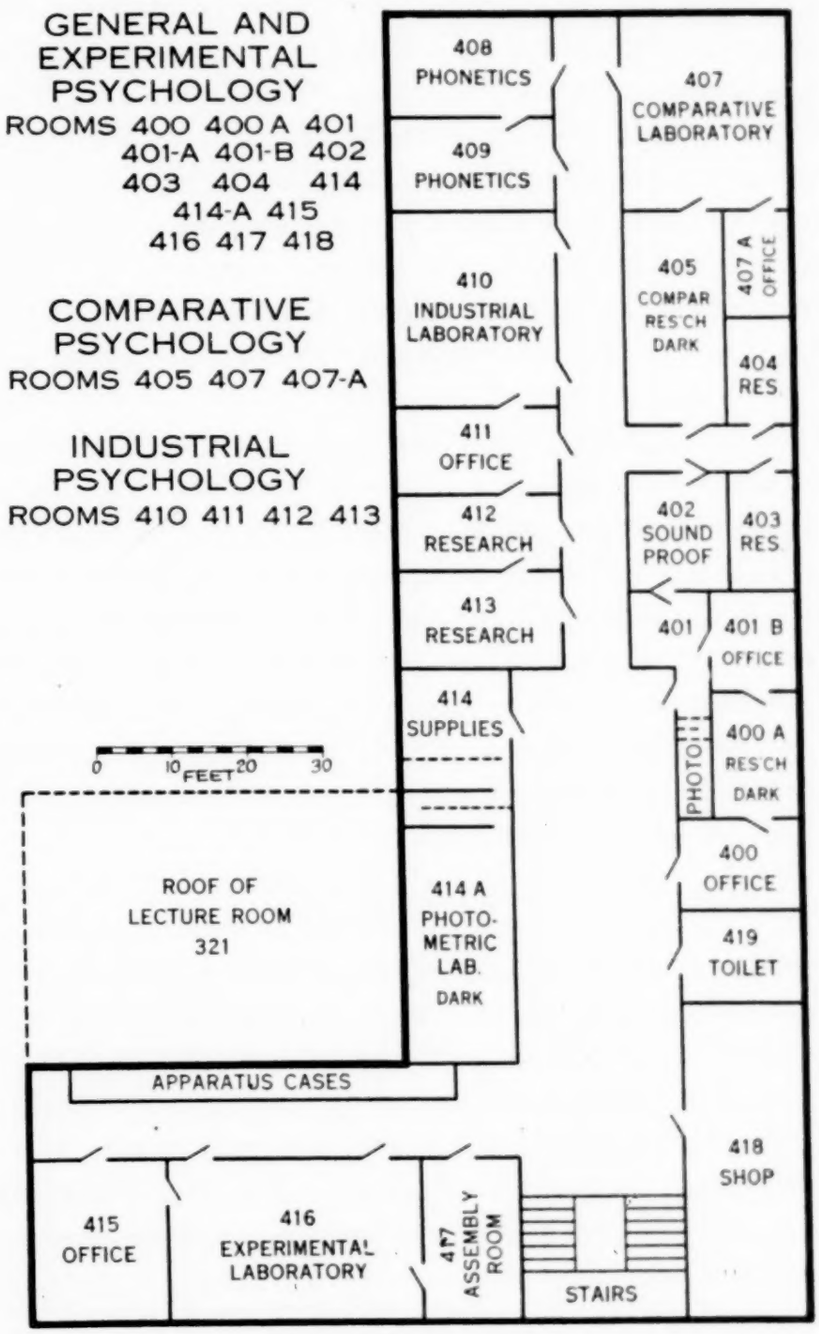

FIG. 3

At present only the advanced classes and the investigations are carried on in this building. The floor space (Figs. 2 and 3), not including approaches, toilets, janitor rooms, etc., for

the advanced courses, is 7,008 square feet on the fourth floor and 6,282 square feet on the third floor, a total of 13,290 square feet. In addition, the larger advanced courses require the part-time use of four lecture rooms located on the first, second and third floors. To this are to be added the lecture rooms, class demonstration equipment-room, and offices for the instructors of the elementary courses, which are now in another building (University Hall). Combining the elementary and advanced class rooms and laboratories gives a floor space of approximately 50,000 square feet. When the proposed extensions have been added to the Education Building there will be enough class rooms to handle both the elementary and advanced classes in one building.

Room 300 is designed for a general office for elementary instructors and assistants not assigned to any particular division. The departmental stenographer, files, stationery, mimeographing machine, etc., are located in Room 325. The head of the department has his office in Room 325 A.

The plans shown in Figs. 2 and 3 have been simplified so as to show merely the space relations. The architectural features, windows, plumbing, etc., have been omitted. All rooms, including dark rooms, have washed artificial ventilation, which in summer is water-cooled.

The Library

The library is located on the second floor, but has not yet been fully equipped. When ready for use, it will carry all the bound and current volumes of the periodicals, and the books that are specifically psychological in character. It will be in charge of an efficient librarian who is familiar with the needs of graduate students and instructors. The larger demands for the students in the elementary courses will be met by the general University Library.

General Equipment

The general equipment for demonstration, research, precision instruments, permanent shop-machine tools, calculating machines, typewriters, mimeograph outfit, lanterns, screens, etc., are inventoried at about $25,000.

The shop, located in Room 418, is in charge of Mr. I. L. Hampton, an expert in apparatus construction and design. The shop equipment consists of the following:

A small Pratt-Whitney bench lathe for wood and brass. A Dalton engine lathe; swing 9 inches, compound rest, lead screw, feed rod. A Bickett bench-type milling machine; working surface of table $3\frac{1}{2}$ x 16 inches, six-inch universal index centers, three index plates giving divisions from 1 to 100, swivel vise. A United States sensitive drill press, $\frac{1}{2}$-inch capacity. A 26-inch Crescent band saw with $\frac{3}{8}$-inch blade. A standard electric grinder. A Black & Decker portable electric drill. All these tools are driven by independent motors.

In addition there is the usual equipment of benches, hand tools, soldering outfit, drawing outfit, gauges, supplies, etc., necessary for developing and constructing experimental set-ups.

The general switch-board and battery room is also located in the shop. The storage batteries are sufficient in capacity to deliver 120 volts, at four or five amperes, for a few hours at a time. As a rule such high voltage battery current is seldom used. For high voltage direct current, there is a 3 K.W. motor-generator which delivers direct current at 110 volts. This is a temporary installation and will be replaced by a 7 K.W. motor-generator unit.

The switch board is so arranged that any combination of battery voltage from two to one hundred and twenty can be delivered to the rooms on the third and fourth floors.

In each room or office that is likely to be used for research purposes there are one or more 'triplets' of wall sockets. Each of the sockets in a triplet is supplied with a refillable fuse plug which ordinarily carries a three-ampere fuse. This is sufficient for any ordinary piece of apparatus but the fuse will blow out before any serious damage is done through accidental short-circuits or errors in wiring. For the more delicate precision instruments, a fuse corresponding to the sensitivity of the instrument is used.

One of the three sockets in each triplet carries 110-volt

A.C., the second 110-volt D.C., and the third leads to the switch board in the shop from which it may be connected with any other room or triplet in the circuit. This makes it possible to interconnect every room with every other room and to secure any signal or apparatus combination between any sets of rooms.

In addition to the triplet wall sets, drop sockets carrying 110-volt A.C., independently of the general illumination, are suspended from the ceiling in all of the laboratories and larger research rooms. These sockets are for small motors or special illumination, and avoid the nuisance of connections dragging on the floor to the wall plugs. This is convenient when a number of students are working together on the routine laboratory experiments.

All rooms that have a wall in common have so-called 'room to room' openings. Such an opening is one foot square, about five feet above the floor, closed on one side by a hinged door. The object of the opening is to make it possible to string wires and tubes from one room to the next without cutting holes into the doors.

The number of dark rooms is relatively large, partly as the result of the type of architecture and partly by design. It has been found that, for many experiments, dark rooms are preferred to daylight rooms, on account of the ease with which the illumination can be controlled. With a forced ventilating system in every room, the difficulty of 'stuffiness' is overcome. The research rooms are not assigned to any division. Gas and water connections are found in all rooms that need them, and there are telephone extensions to all offices.

Division Reports

The special reports which follow were prepared by the personnel of the respective divisions.

1. *Division of General, Experimental and Comparative Psychology*

Elementary Psychology. The class rooms and offices of the instructors are placed on the fourth floor of University

Hall which is not shown in the illustrations. In a typical quarter, the elementary psychology is divided into twenty sections of from thirty-five to forty students each. Instruction is predominantly through class discussion. A syllabus, used in all sections, is designed to provide for the necessary unity without hampering the initiative of the individual instructors. Throughout the course, emphasis is placed upon scientific method, on the logical foundations of psychological systems, and on the relations between psychology and other sciences. No single text meets these needs, and therefore an extensive study is made of seven other books in addition to some common text-book. The collection of charts, models, and demonstration apparatus is entirely distinct from the equipment of the experimental and advanced courses. Special rooms have been set aside for assembling the more complicated demonstrations.

Experimental Psychology. The office is located in Room 415. Room 416 is the laboratory, equipped with small tables which can be combined into larger units of any size. The apparatus for the routine experiments is kept in a special case against the wall. The room is used for the preliminary lectures and for those experiments which do not require special conditions. It will accommodate about twenty-five students for a lecture and about ten for laboratory work. For the routine experiments requiring quiet, special illumination or complicated apparatus, the other rooms on this floor are used. The research precision instruments are kept in a large apparatus case in the hall opposite Rooms 415 and 416.

No effort is made to restrict the researches in this division to a specific program. The problems in experimental psychology seem to be expanding beyond the introspective technique designed to investigate the attributes of the mind and the more exclusively individual differences between adults, so as to include the study and isolation of those variables, both environmental and sensorimotor, through which the infant becomes a coöperative unit in a social organization. Concretely, this leads to fundamental research in genetic, infantile,

and child behavior; the acquisition and nature of language; learning, thinking, inhibition, etc. This tendency places more stress on the theoretical and systematic principles underlying psychology.

In general the research students are expected to do much of the manual work on their apparatus. This training is regarded as essential, especially for those preparing to teach psychology. After the student has worked out the preliminary apparatus, it is redesigned in consultation with his advisor and the technician and a more permanent and workmanlike apparatus is made.

The Comparative Laboratory is located in Room 407. It has a cement floor which may be flushed with a hose and is equipped with a large lead-lined sink. Room 405 is used as a special experimental room for animals. Room 402 is partly sound-proof. It has double doors and for any experiment requiring the complete absence of sound another room may be built inside it. Room 414A is the photometric room, long and narrow with light-trap doors.

2. *Division of Abnormal and Clinical Psychology*

This division occupies ten rooms on the third floor east, and comprises the psychological clinic for normal and abnormal children of all ages and for adults, the student consultation service and the instructional work. Room 303 is a waiting-room for those who bring cases to the clinic, and an appointment office with telephone. Rooms 304A and 305 are used as office and for private consultation. The laboratory is located in Room 304 and serves as a small class room. It has work tables and equipment for vocational guidance and personality analysis, as well as for research and training in clinical procedure. The apparatus consists of standard and special test sets, spirometers, scales, stadiometer, psychometer, micro-projector, reflecting lantern, microscope, etc. There are cases for periodicals, books, test blanks, and records, and for general filing.

Rooms 315, 316, 317, 318, 319 are furnished and equipped for individual examinations in the clinic and for laboratory

instruction through individual practice in clinical procedure. They are also used in the student consultation service which has its headquarters in Room 312.

3. *Division of Educational Psychology*

Room 301 is the office, and contains the exhibits of tests. Off this room there is a large storage closet containing equipment, tests, filing cases and cabinets. Room 302 is the laboratory. Room 320 is an office and research room.

Three general types of work in educational psychology are at present being stressed. (1) Study and research in the field of mental and educational tests. The division is maintaining a complete collection and exhibit of tests and related materials, thus offering special opportunities for the study of the testing movement in many of its phases. A large stock of the best present tests are kept on hand for experimentation in this field. (2) Investigations in learning, study habits, diagnosis and remedy of children's difficulties in various school subjects, the development of drill materials, and special techniques in child study. (3) The development and experimentation with automatic devices for testing, drill and learning.

4. *Division of Industrial Psychology*

Room 410 is the laboratory class room and is equipped with tables, demonstration-bench with water, gas and electrical outlets. The office, Room 411, provides for the professor and assistant, and desk space for advanced students. Rooms 412 and 413 are devoted to research and have the usual equipment that goes with the other research rooms.

The laboratory training and research program takes three principal directions.

(1) Advertising and selling. A consideration of the outstanding techniques used in the selling organizations both as laboratory and field problems. (2) Personnel. Investigations in the effective selection of employees. The adjustment of the worker to his work by the use of occupational tests, rating scales, job analysis and the statistical methods necessary for dealing with such data. (3) Industrial effi-

ciency. Studies in industrial learning, economy of movement, fatigue, monotony, industrial harmony, environmental conditions, motives and incentives and the problems of production.

This division has the apparatus, such as chronoscopes, tachistoscopes and special equipment, suited to its program, a supply of material adapted to various advertising problems, and a considerable collection of specimen occupational tests and rating scales.

5. *Divisions of Statistics and University Intelligence Tests*

Rooms 322, 323, 324, 324A, and 324B comprise a suite used for the statistical work of the Department, and for the University intelligence tests and personnel research work.

The research program of the division is directed toward establishing the assumption that man's behavior may be analyzed into a rather restricted number of unitary capacities and abilities which may be reliably measured toward the end that one generalized equation may be used to predict the individual's performance in all the important major lines of human endeavor. The equipment includes the records of some 25,000 students, which have accumulated in the office of the University intelligence test secretary since 1919. The University intelligence tests have been constantly revised and now include the individual units which have been found most significant. To increase the reliability of diagnosing probable academic and occupational success, twenty sets of the Minnesota edition of the Stenquist Assembly test for mechanical ability have been added. The special statistical equipment includes five electric and one hand Monroe calculators, one standard deviation machine, one demonstration slide rule, one paired-comparison machine, one drawing board and drafting outfit, twenty sets of Barlow Tables, one blackboard with cross section rulings, one Dalton multiplex electrically driven adding machine, one rotating table for plotting correlations on polar coördinate paper, one typewriter with statistical keyboard, and the necessary filing cabinets.

Coöperation between the Divisions

No attempt is made to differentiate the research problems into rigid categories. The graduate student selects his major professor and with him works out the details of his courses and the method of dealing with his particular problems. The resources of all divisions are available to him and he is encouraged to use them.